PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXII.

PART I.–II

LONDON:

PRINTED BY W. NICOL, SUCCESSOR TO W. BULMER AND CO.
CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, PRINTERS TO THE
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APPENDIX.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation,

1. To Captain EDWARD SABINE, for his various Communications to the Royal Society relating to his researches made in the late expedition to the Arctic regions.

2. To JOHN FREDERICK WILLIAM HERSCHEL, Esq. for his Papers printed in the Philosophical Transactions.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Bakerian Lecture. An Account of Experiments to determine the amount of the Dip of the Magnetic Needle in London, in August* 1821; *with Remarks on the Instruments which are usually employed in such determinations. By Captain* EDWARD SABINE *of the Royal Regiment of Artillery, F. R. S.*

Read November 22, 1821.

THE increased attention which has been given of late years by several philosophers to the subject of *magnetism*, and the consequent advance which has been made in this branch of natural knowledge, render it desirable, that a greater degree of accuracy should be obtained in all respects, in observing its various terrestrial phenomena, than hitherto.

This remark applies especially to observations on the dip of the needle; the instruments in general use for this purpose have received little or no improvement during the last fifty years, and produce results which can only be considered as approximate, even when the observer has made himself well acquainted with the various sources of inaccuracy in the instrument, and has adopted precautions to guard against, or remedy them.

Many of these, indeed, are not difficult of detection, and admit of compensation by certain known methods of observing; such are those which are caused by incorrect graduation, by the eccentricity of the needle with respect to the divided circle, by the agate planes which support the axis of the needle not being truly horizontal, or by their non-coincidence with the right line joining the zeros of the circle. The errors to which even the most careful determinations are subject, are to be referred chiefly to faults in the construction of the needle itself; 1st, to imperfection in the axis, whereby the needle, on being made to oscillate, will not return with certainty in repeated trials to the same division of the limb; and, 2ndly, to the difficulty which even the most skilful artists experience, in the endeavour to make the axis of motion pass through the centre of gravity; a condition which is essential to accuracy in the usual mode of observation, but of which it may be safely said, that its accomplishment admits of no very certain proof, and that it is rarely or never succeeded in.

It is obvious that a needle, of which the balance is not thus correctly adjusted, will not assume, on being suspended freely in the plane of the meridian, the direction which magnetism alone would have imparted to it, and that it will, in consequence, err sensibly from the true dip; the remedy which has been recommended for this inconvenience, and which has become the usual practice, is to reverse the poles of the needle, and to take the arithmetical mean of the arcs indicated in four positions, as the true magnetic dip. The sanction of this method by persons who are regarded as authorities, is a sufficient indication, that observations of the dip

were considered by them merely as approximations; since it could not have been otherwise overlooked, that the arithmetical mean is in no case the result which is strictly deducible from the arcs, and that, in many instances, it must differ considerably from the more correct deduction; whilst the adoption of this method in general practice, has charged the results with an error, which might have been avoided; the amount of which can only now be known, when the details of the observation have been given, and furnish the means of re-computation.

The perfect balancing of the needle is sometimes attempted by a cross of wires affixed to the axis, as described in the Philosophical Transactions for 1772, Article 35; but this contrivance is more ingenious than useful in practice, and introduces a liability to errors, of far more importance than the inconveniencies which it was designed to obviate. The adjustment of the balance, after the needle is magnetised, is in itself a troublesome, tedious, and uncertain operation, and is far too subject to derangement to be confided in, when the instrument has been removed from station to station; moreover, the inaccuracies occasioned by friction, are augmented by the additional weight of the mechanism on the one arm of the axis, and of a counterweight on the other. The errors of an imperfect balance are reducible by calculation, but those of friction are not so; whether they may arise from the axis not being truly cylindrical, or from the inequalities of its surface producing a resistance on the planes, which the moving force of the needle is not fully adequate to overcome. It may be, therefore, justly remarked, that the true and unimpeded motion of the axis, and the consequent

return of a needle in successive trials to the same division of the limb, is one of the principal qualifications of a good dipping needle.

Having been convinced by the trial of several needles, that the disagreement in their results was chiefly to be attributed to the various causes of inaccuracy in the motion of the axis, I requested Mr. DOLLOND to make a needle on a construction suggested, from similar experience, by Professor J. TOBIAS MEYER, in his treatise "de usu accuratiori acûs inclinatoriæ Magneticæ," published in the Transactions of the Royal Society of Sciences at Göttingen for 1814. The experiments which are now submitted to the Royal Society were made with this needle, which, for simplicity of construction, convenience in use, and consistency of results, appears to deserve a preference over those which have been hitherto employed.

As the needle which Mr. DOLLOND made, differed in some few particulars from the construction recommended by Professor MEYER, it may be proper to prefix a short description of it, as well as of the mode of observation.

The needle is a parallelopipedon of eleven inches and a half in length, four tenths in breadth, and one twentieth in thickness; the ends are rounded; and a line marked on the face of the needle passing through the centre to the extremities, answers the purpose of an index.

The cylindrical axis on which the needle revolves, is of bell metal, terminated, where it rests on the agate planes, by cylinders of less diameter; the finer these terminations are made, so long as they do not bend with the weight of the needle, the more accurate will be the oscillations; small

grooves in the thicker part of the axis receive the Y's, which raise and lower the needle on its supports, and ensure that the same parts of the axis rest in each observation on the planes.

A small brass sphere traverses on a steel screw, inserted in the lower edge of the needle, as nearly as possible in the perpendicular to the index line passing through the axis of motion; by this mechanism, the centre of gravity of the needle, with the screw and sphere, may be made to fall more or less below the axis of motion, according as the sphere is screwed nearer or more distant from the needle, and according as spheres of greater or less diameter are employed.

The object proposed in thus separating the centres of motion and gravity, is to give to the needle a force arising from its own weight to assist that of magnetism in overcoming the inequalities of the axis, and thus to cause the needle to return, after oscillation, with more certainty to the same point of the divided limb, than it would do were the centres strictly coincident.

The centres of motion and of gravity not coinciding, the position which the needle assumes, when placed in the magnetic meridian, is not that of the dip: but the dip is deducible, by an easy calculation, from observations made with such a needle, according to the following directions.

If the needle has been carefully made, and the screw inserted truly as described, the centres of motion and of gravity will be disposed as in the lever of a balance, when a right line joining them will be a perpendicular to the horizontal passing through the extremities (or to the index line); this

condition is not indeed a necessary one, but it is desirable to be accomplished, because it shortens the observations, as well as the calculation, from whence the dip is deduced; its fulfilment may be ascertained with great precision by placing the needle on the agate planes before magnetism is imparted to it, and observing whether it returns to a horizontal direction, after oscillation in each position of the axis; if it does not, it may be made to do so at this time with no great trouble.

With a needle in which this adjustment can be relied on, two observations made in the magnetic meridian are sufficient for the determination of the dip, the two faces of the needle being successively towards the observer, reversing the position of the axis on its supports in such manner that the edge of the needle which is uppermost in the one observation becomes lowermost in the other; the angles which the needle makes with the vertical in these two positions being read, the mean of the tangents of those angles is the co-tangent of the dip.

But when needles are used in which this adjustment has not been made, or where its accuracy cannot be relied on, four observations are required; two being those which are already directed; the two others are similar to them, but with the poles of the needle reversed; calling then the first arcs F and f, and those with the poles reversed G and g, and taking

$$\text{tang } F + \tan f = A$$
$$\text{tang } F - \tan f = B$$
$$\text{tang } G + \tan g = C$$
$$\text{tang } G - \tan g = D$$

$$\frac{A \cdot D}{B + D} + \frac{B \cdot C}{B + D} = \text{twice the co-tangent of the dip.}$$

The demonstration of this formula is given in the treatise which has been referred to; the investigation is simple.

In reversing the poles, it is not necessary that the magnetic force imparted to the needle should be the same in amount as it possessed previously to the operation.

By adopting the precaution of placing the needle in a groove to prevent its lateral motion, and by confining the sides of the magnet by parallel strips of wood, so that in moving along the needle they may preserve its direction, the poles may always be ensured to coincide with the extremities of the longitudinal axis.

It is desirable to be furnished with several spheres of different diameters, to possess the means of proportioning the force, arising from the eccentricity, to the force of magnetism, as the latter is doubled between the equator and the poles; it may be expedient that the force of magnetism should always predominate, though no such inference appears to result from the present experiments, in which eight spheres were used, numbered according to size, No. 1 being the largest.

If the distance between the centres of motion and of gravity be considerable, the arcs in the alternate observations will be on different sides of the vertical, especially where the dip is great; in such cases the arcs to the south of the vertical are read negatively.

The instrument in which the needle was tried is already described in the Philosophical Transactions for 1819, page 132, and several improvements which have been since added, in the Appendix to Captain Parry's Voyage of Discovery, pages cvii. cxxxix., &c.; the perfect horizontality of the

agate planes, and the adjustments of the zeros of the circle were verified in every change of position of the instrument by the double cone apparatus described in page cxl., which proves very convenient in use, and is highly conducive to accuracy; the circle is divided into spaces of twenty minutes, but by means of a moveable lens, the arcs to which the needle settles can be read off, with tolerable correctness, to minutes; the arcs, in each of the four positions forming the elements from whence the dip is deduced, are the arithmetical mean of (usually) six observations, half of which are with the face of the circle towards the east, and half with the face towards the west; the needle being lifted by the Y's and lowered gently on its supports between each observation; the arcs indicated by both ends of the needle are also read, to correct the errors arising from inequality in the divisions, or from the axis of the needle not passing correctly through the centre of the circle.

The experiments were made in the nursery garden in the Regent's Park, by permission of Mr. JENKINS, the proprietor. The situation is in all respects an eligible one, being far removed from the neighbourhood of iron.

The two first experiments are given in detail, that the method of observation may be more fully exemplified.

Experiment 1. *August* 3*d*, 1821, with sphere No. 8, midway between the edge of the needle and the end of the screw. In this experiment all the arcs were on the north side of the vertical, and were therefore all read positively.

	Face A of the Needle towards the Observer. N. end	Face A of the Needle towards the Observer. S. end	Face B towards the Observer. N. end	Face B towards the Observer. S. end
Face of the Circle East	31° 00′	30° 52′	9° 18′	9° 18′
	31 00	30 52	9 17	9 15
	31 00	30 52	9 20	9 19
	31 02	30 55	9 24	9 22
	31 00	30 52	9 24	9 23
	31 02	30 56	9 19	9 18
Face West	31 25	31 20	9 25	9 30
	31 20	31 13	9 25	9 30
	31 20	31 13	9 22	9 22
	31 22	31 15	9 21	9 22
	31 24	31 17	9 25	9 30
	31 26	31 20	9 25	9 30
	31 11,7	31 04,8	9 22,1	9 22,4
	31 08,2 = F		9 22,3 = *f*.	

Poles Inverted.

Face West	30° 15′	30° 05′	5° 20′	5° 23′
	30 12	30 00	5 17	5 20
	30 10	30 00	5 20	5 23
	30 10	30 00	5 22	5 26
	30 10	30 00	5 22	5 27
	30 16	30 04	5 18	5 22
Face East	29 20	29 10	7 15	7 10
	29 21	29 11	7 12	7 12
	29 21	29 12	7 13	7 15
	29 19	29 08	7 13	7 14
	29 24	29 17	7 14	7 14
	29 19	29 08	7 14	7 14
	29 46,4	29 36,3	6 16,6	6 18,3
	29 41,3 = G		6 17,4 = *g*	

Here $\tan F = \tan 31^\circ\ 08',2 = 0,60411$; and $\tan G = \tan 29^\circ\ 41',3 = 0,57012$

$\tan f = \tan\ 9\ 22,3 = 0,16504$; $\tan g = \tan\ 6\ 17,4 = 0,11022$

$\tan F + \tan f = A = 0,76915$; $\tan G + \tan g = C = 0,68034$

$\tan F - \tan f = B = 0,43907$; $\tan G - \tan g = D = 0,4599$

$B + D = 0,89897$

And $\frac{AD}{B+D} + \frac{BC}{B+D} = \frac{0{,}76915 \times 0{,}4599}{0{,}89897} + \frac{0{,}68034 \times 0{,}43907}{0{,}89897} = 0{,}39348$ $+ 0{,}33229 = 0{,}72577$ = twice the cotangent of the dip; therefore, $\frac{0{,}72577}{2} = 0{,}36288$; cot. of $70° \; 03', 3$ north dip.

Experiment 2. *August 6th*, 1821, with sphere No. 1, screwed up close to the needle; in this experiment the alternate arcs were on the south side of the vertical, and were therefore read and used negatively.

	Face A towards the observer.		Face B towards the observer.	
	N. end ° ′	S. end ° ′	N. end ° ′	S. end ° ′
Face of the Circle East	49 20	49 00	—22 14	—22 06
	49 20	49 00	—22 14	—22 06
	49 22	49 02	—22 17	—22 12
Face West	49 22	49 04	—22 40	—22 20
	49 22	49 03	—22 40	—22 20
	49 22	49 04	—22 38	—22 18
	49 21,3	49 02,1	—22 27,1	—22 17
	49 11,7 = F		— 22 22 = *f*.	

Poles inverted.

	° ′	° ′	° ′	° ′
Face West	47 15	47 00	—20 20	—20 10
	47 20	47 00	—20 20	—20 08
	47 14	46 56	—20 20	—20 04
Face East	47 00	46 40	—20 10	—20 00
	47 00	46 40	—20 10	—19 58
	47 00	46 40	—20 10	—19 58
	47 08,1	46 49,3	—20 15	—20 03
	46 58,7 = G		— 20 09 = *g*	

Here tan F = tan 49 11,7 = 1,15831; and tan G = tan 46 58,7 = 1,07156
tan *f* = tan — 22 22 = — 0,41149 tan *g* = tan — 20 09 = — 0,36694
tan F + tan *f* = A = 0,74682 tan G + tan *g* = C = 0,70462
tan F — tan *f* = B 1,5698 tan G — tan *g* = D = 1,4385

B + D = 3,0083

$$\frac{AB}{B+D} + \frac{BC}{B+D} = \frac{0{,}74682 \times 1{,}4385}{3{,}0083} + \frac{0{,}70462 \times 1{,}5698}{3{,}0083} = 0{,}35711$$

$+ 0{,}36769 = 0{,}7248$; $\frac{0{,}7248}{2} = 0{,}3624$ cot. of 70 04,7 N. dip.

Abstract of ten experiments with MEYER'S *needle.*

Exp. 1. Aug. 3.	Sphere 8 screwed half up.	Marked end of the Needle being *a*.	N. Pole; F = 31° 08′,2 and S. Pole; G = 29 41,3	*f* = + 9° 22′,3 *g* = + 6 17,4	7
Exp. 2. Aug. 6.	Sphere 1 close to the Needle.	———	N. Pole; F = 49 11,7 S. Pole; G = 46 58,7	*f* = − 22 22 *g* = − 20 09	7
Exp. 3. Aug. 6.	Sphere 8 nearly as in the 1st Experiment.	———	N. Pole; F = 30 36,8 S. Pole; G = 28 47,7	*f* = + 10 08,3 *g* = + 7 46,6	7
Exp. 4. Aug. 11.	Sphere 3 close to the Needle.	———	N. Pole; F = 45 58 S. Pole; G = 41 50,7	*f* = − 14 49,1 *g* = − 11 28,7	7
Exp. 5. Aug. 13.	Sphere 7 close to the Needle.	———	N. Pole; F = 27 24,3 S. Pole; G = 24 14,2	*f* = + 14 07,2 *g* = + 13 21,7	7
Exp. 6. Aug. 13.	Sphere 6 close to the Needle.	———	N. Pole; F = 30 36,2 S. Pole; G = 27 12,6	*f* = + 10 17,2 *g* = + 9 15,3	7
Exp. 7. Aug. 15.	Sphere 5 close to the Needle.	———	N. Pole; F = 32 00,2 S. Pole; G = 28 57,4	*f* = + 8 03,4 *g* = + 7 40,9	7
Exp. 8. Aug. 15.	No Sphere; weight of the screw close.	———	N. Pole; F = 24 14 S. Pole; G = 22 17,5	*f* = + 17 34,1 *g* = + 15 34,8	7
Exp. 9. Aug. 20.	Sphere 1 close to the Needle.	———	N. Pole; F = 48 24,7 S. Pole; G = 44 57,1	*f* = − 19 25 *g* = − 17 19	7
Exp. 10. Aug. 20.	Sphere 7 close to the Needle	———	N. Pole; F = 24 27,6 S. Pole; G = 22 04	*f* = + 17 38,5 *g* = + 15 22	7

Dip in London, August 1821 = 7

Note. The screw on which the spheres traversed was made, in the first instance, half an inch in length, but was shortened one half after the third experiment; being found still longer than necessary, it was again shortened after the eighth experiment, until its length just equalled the diameter of the largest sphere.

Being desirous to confirm the correctness of the result obtained with MEYER's needle, I made the following experiments for the purpose of deducing at least an approximation of the dip by a method suggested, I believe, originally by LAPLACE, of observing the times in which a certain number of oscillations are made by a dipping needle, in the magnetic meridian, and in the plane perpendicular to it.

The force acting on the needle in the latter case is reduced, according to the principles of the resolution of forces, in the ratio of the radius to the sine of the dip; whence calling M the time in which a certain number of vibrations are made in the meridian, P the corresponding time in the perpendicular plane, and D the dip, $\frac{M^2}{P^2} = \text{sine } D$.

In all the subsequent experiments, the needles were retained and released at an angle of 40° with the meridian, by an apparatus for that purpose fitted to the instrument, and were suffered to oscillate until the arc of vibration was reduced to 30° before the account was taken up, and the observation commenced.

Exp. 1. *Sept.* 3d, with a dipping needle, the centre of gravity of which was rendered nearly coincident with the axis of motion, by a slider of silk advanced towards the end which was previously too light, until the needle in the meridian stood at 70° nearly, and became vertical when the instrument was moved 90° in azimuth.

In the meridian.

Oscill.	1 Arc.	Time.		Int.	2 Arc.	Time.		Int.	3 Arc.	Time.		Int.
	o	m	s	s	o	m	s	s	o	m	s	s
0	30	0	00		30	0	00		30	0	00	
				55,5				55				55,5
10	24	0	55,5		23	0	55		24	0	55,5	
				55				55				55
20	20	1	50,5		20	1	50		21	1	50,5	
				55				55				55
30	17	2	45,5		16	2	45		17	2	45,5	
				54,5				54,5				55
40	14	3	40		13	3	39,5		14	3	40,5	
				54				54,5				54,5
50	10	4	34		10	4	34		10	4	35	
50 Oscillations in		274				274				275 seconds.		

M = 274,33.

Perpendicular to the meridian.

Oscill.	1 Arc.	Time.		Int.	2 Arc.	Time.		Int.
	o	m	s	s	o	m	s	s
0	30	0	00		28	0	00	
				57				57
10	25	0	57		22	0	57	
				57				57
20	20	1	54		18	1	54	
				56,5				56,5
30	16	2	50,5		16	2	50,5	
				56,5				56,5
40	14	3	47		14	3	47	
				56				56
50	11	4	43		10	4	43	
50 Oscillations in		283 ……				…… 283 seconds.		

P = 283

$\frac{M^2}{P^2} = \frac{274{,}33^2}{283^2} = \cdot 93966$ sine of $69^\circ.59',7$. N. dip.

Experiment 2. *Sept.* 7*th*, with a dipping needle, (No. 2,) balanced by a cross of wires affixed to the axis.

In the meridian.

Oscill.	1 Arc.	Time.		Int.	2 Arc.	Time.		Int.	3 Arc.	Time.		Int.
	°	m	s	s	°	m	s	s	°	m	s	s
0	30	0	00		30	0	00		30	0	00	
10	22	0	50	50	21	0	50	50	25	0	50	50
20	17	1	39,5	49,5	16	1	39	49	18	1	39	49
30	13	2	28	48,5	12	2	27,5	48,5	12	2	28	49
40	10	3	17	49	9	3	17	49,5	10	3	17	49
50	6	4	06	49	6	4	06	49	7	4	06	49
60	4	4	46	50	4	4	56,5	50,5	5	4	56	50
70	2	5	46	50	2	5	46,5	50	3	5	46	50
70 Oscillations in			346				346,5				346 seconds.	

M = 346,17

Perpendicular to the meridian.

Oscill.	1 Arc.	Time.		Int.	2 Arc.	Time.		Int.	3 Arc.	Time.		Int.
	°	m	s	s	°	m	s	s	°	m	s	s
0	30	0	00		30	0	00		30	0	00	
10	22	0	51	51	23	0	50,5	50,5	23	0	52	52
20	15	1	41,5	50,5	18	1	42	51,5	17	1	43	51
30	10	2	33,5	52	12	2	33	51	12	2	34	51
40	8	3	24,5	51	8	3	24	51	9	3	25	51
50	6	4	15,5	51	6	4	15	51	6	4	17	52
60	4	5	06,5	51	4	5	06	51	3	5	07	50
70	2	5	17	50,5	2	5	57	51	2	5	57	50
70 Oscillations in			357				357				357 seconds.	

P = 357.

$$\frac{M^2}{P^2} = \frac{346{,}17^2}{357^2} = \text{'94025 sine of } 70^\circ\, 05',8 \text{ N. dip.}$$

Exp. 3, *Sept.* 7, with a dipping needle (No. 3.) made by Mr. Dollond, the arms of which were conical, having at their common base a small cube perforated to receive the axis; the cylindrical terminations of the axis were similar to those of Meyer's needle, very slender, and very carefully turned.

In the meridian.

Oscill.	1 Arc.	1 Time.	1 Int	2 Arc.	2 Time.	2 Int.	3 Arc.	3 Time.	3 Int.	4 Arc.	4 Time.	4 Int.
	o	m s	s	o	m s	s	o	m s	s	o	m s	s
0	28	0 00		27	0 00		27	0 00		28	0 00	
10	23	0 37	37	22	0 38	38	21	0 37,5	37,5	23	0 37	37
20	19	1 15	38	19	1 15	37	16	1 14,5	37	18	1 14	37
30	16	1 52	37	16	1 52	37	14	1 52	37,5	15	1 51	37
40	14	2 29	37	14	2 29,5	37,5	11	2 29,5	37,5	13	2 29	38
50	12	3 06	37	12	3 07	37,5	8	3 06,5	37	10	3 06	37
60	10	3 43	37	10	3 44	37	6	3 43,5	37	8	3 43	37
70	8	4 20	37	8	4 20,5	36,5	4	4 20,5	37	6	4 20	37
70 Oscillations in		260			260,5			260,5			260 seconds.	

M = 260,25.

Perpendicular to the meridian.

Oscill.	1 Arc.	1 Time.	1 Int.	2 Arc.	2 Time.	2 Int.	3 Arc.	3 Time.	3 Int.	4 Arc.	4 Time.	4 Int.
	o	m s	s	o	m s	s	o	m s	s	o	m s	s
0	30	0 00		27	0 00		27	0 00		28	0 00	
10	27	0 39,5	39,5	22	0 39	39	23	0 38,5	38,5	24	0 39	39
20	22	1 18	38,5	17	1 18	39	19	1 17,5	39	21	1 18	39
30	19	1 57	39	14	1 56	38	16	1 56	38,5	17	1 56	38
40	15	2 35	38	12	2 35	39	14	2 34,5	38,5	15	2 34	38
50	11	3 12	37	8	3 13	38	11	3 12,5	38	12	3 12	38
60	9	3 50	38	6	3 50	37	9	3 50,5	38	10	3 50	38
70	6	4 28,5	38,5	4	4 28	38	7	4 28,5	38	8	4 28,5	38,5
70 Oscillations in		268,5			268			268,5			268,5 seconds.	

P = 268,38.

$$\frac{M^2}{P^2} = \frac{260{,}25^2}{268{,}38^2} = 94033 \text{ sine of } 70^\circ\ 06',5 \text{ N. dip.}$$

Mean of the preceding results.

Experiment 1.	$69^\circ\ 59',7$	$70^\circ\ 04'$ N. dip.
Experiment 2.	$70\ 05,8$	
Experiment 3.	$70\ 06,5$	

This was a nearer accordance with the direct observation with MEYER's needle than I had anticipated. As this method, which is highly deserving of adoption in the lower magnetic latitudes, must become far less certain in parallels so high as 70°, when a very small alteration in either of the observed times will produce a wide difference in the conclusion: unless, therefore, the oscillation of a needle in suitable arcs can be continued through a number of seconds, much exceeding those of the preceding experiments, the result may be liable, without great care and frequent repetition, to considerable error.

I am not aware that a method of deducing the dip on a similar principle, but possessing the same advantages in the high latitudes as the former does between the magnetic equator and 45° (nearly), has been heretofore suggested: it is by observing the times in which a certain number of oscillations are made by the same needle in the following positions; first, when used as a dipping needle, vibrating in the plane of the meridian; and, secondly, when suspended horizontally by a silk thread attached to either end of the axis, the needle being limited thereby to a horizontal motion.

The square of the times of horizontal vibration being increased, as the radius to the *cosine* of the dip, it is obvious, that, as in the former method, the effect of errors of observation on the result increases with the angle of the dip,

according as the differences of the sines progressively diminish; so in this method, on the contrary, the influence of such errors will be lessened in the same ratio: thus in dips of 65° and upwards, the determination may be made with very considerable accuracy, with instruments generally of good construction, and with needles of which the terminations of the axis are very slender.

The horizontal vibrations should be made under a cover of glass, or of wood with glass windows; the silk suspension should be several inches in length, and perfectly free from twist.

The following experiment was made with the needle numbered 3 in the preceding; the silk line was 15 inches in length, and was fastened in a groove near the end of the axis; the oscillations were made in arcs under 25°.

Oscil.	Times. m s	Oscil.	Times. m s		m s	m s
0	0 00	70th	7 25,75		7 25,75	
2nd	0 13	72d	7 38,5		7 25,5	
4th	0 25,75	74th	7 51,25	70 oscil. in	7 25,5	7 25,5
6th	0 38,5	76th	8 04		7 25,5	
8th	0 51,25	78th	8 16,75		7 25,5	
10th	1 04	80th	8 29,25		7 25,25	

Here making $7^{m}\ 25,5^{s} = 445,5^{s} =$ H, and M as before $= 260,25^{s}$, $\frac{M^2}{H^2} = \frac{260,25^2}{445,5^2} = {\cdot}341265$ cosine of 70° 02′,6 N. dip.

The results by the three different methods collected in one view, are as follow, viz:

	° ′
By 10 experiments with Meyer's needle -	70,02,9
By the times of oscillation in the magnetic meridian and in the plane perpendicular to it; mean by three needles.	70,04
By the times of vertical and horizontal oscillation	70,02,6

Whence 70° 03′ may be considered as the mean dip of the needle towards the north in the Regent's Park, in August and September 1821, within four hours of noon, being the limit within which all the experiments were made.

In referring to the observations which are recorded to have been made for the purpose of determining the dip in London in former years, those of Mr. NAIRNE, in 1772, of Mr. CAVENDISH, in 1776, and of Mr. GILPIN, in 1805, appear to have received, and to be deserving of, principal consideration; the errors by which these several determinations may have been affected, in consequence of the imperfections of the instruments, may be believed to have been confined within limits of no great extent, by the method of the observers, and by the precautions which they adopted; as, however, the observations were made in houses in close built parts of the metropolis, they were all subject to the influence of local attraction, from whence may have originated errors of greater consequence possibly than those of the instruments; nor can the application of a correction found by observing the difference of the dip, on the outside of the house, be considered an effectual remedy, inasmuch as the needle may still have been attracted by iron in the adjoining houses, or in the neighbourhood. It needs only to try needles in different situations in a city, to be convinced how little dependance should be placed in the accuracy of such results; it may, no doubt, be principally owing to this cause, rather than to instrumental error, that the dip at the Apartments of the Royal Society is stated in the Philosophical Transactions for the present year (1821), to be 71° 6′ or 71° 42′.

As the observations of Mr. NAIRNE, in 1772, and of Mr. CAVENDISH, in 1776, do not differ very widely from each

other, either in the date, or in the amount of the dip, their mean, 72° 25′, in 1774, may be considered as the best approximation which can now be made to a knowledge of the amount of the dip in London at an early period.

By comparing this amount with the dip in the present year as above determined, we obtain 3′,02 as a mean annual rate of diminution between 1774 and 1821; which is less by two-fifths than the mean annual diminution at Paris between the years 1798 and 1814, as deduced from the observations of Messrs. Humboldt, Gay Lussac, and Arago; whence it might be inferred, if sufficient dependance could be placed on the accuracy of the observations, that the annual variation of the dip in this part of the world, is greater now than it was 30 or 40 years since.

It is, however, worthy of notice, as being at least a curious coincidence, that if we take Mr. Whiston's determination of the dip in 1720, 75° 10′ (of which Mr. Cavendish remarked in the Philosophical Transactions for 1776, Art. 21,) that "he believed it to have been pretty accurate, as Mr. Whiston observed in many parts of the kingdom, and his observations agreed well together," we obtain between the years 1720 and 1774, an annual diminution of 3′,05, which differs only three hundredths of a minute from the rate which has been now found for the succeeding 47 years.

It may not be useless, briefly to examine how far the knowledge of the amount of change, by direct observation, is capable of receiving confirmation by the effect which a diminution of dip must produce on the vibrations of a needle suspended horizontally.

If the intensity of the magnetic force be considered to vary

in the ratio suggested by Dr. YOUNG, inversely as the square root of four diminished by three times the square of the sine of the dip, which ratio has been remarkably confirmed in dips from 70 to 90 degrees, by the experiments made during the late Arctic voyage; the force acting on the horizontal needle, being reduced as the radius to the cosine, becomes inversely as $\sqrt{\frac{1}{1-ss}+3}$; s being the sine of the dip; whence, in London, the duration of any number of horizontal vibrations would be increased by about $\frac{1}{3000}$ part, on a reduction of one minute in the dip.

The needle No. 3. of the preceding experiments, suspended in the manner therein described, and released at an arc of 40° from the meridian, will continue to vibrate more than forty minutes, making upwards of 400 vibrations, before the arcs become so small as to render the completion of each vibration indistinct. If the times of commencement and conclusion are observed in the method exemplified in the experiment with this needle, page 17; the duration of any number of vibrations may be readily and accurately determined to a part of a second; if, therefore, 400 be taken as the experimental number, and the duration be supposed 42 minutes, or 2520 seconds, the reduction of 3 minutes of dip, which is presumed to take place annually, would cause an increase of two seconds and two-tenths in the time of vibration; which difference may, perhaps, be considered sufficient to encourage the experiment, especially if a mean be taken of many observations in each year; in which case it may be advisable to compare together observations made at the same season in successive years; and perhaps, also, at the same hour of the day; although the experiments of Messrs. HUMBOLDT and

Gay Lussac have shown, that if any hourly variation of the force does obtain, it is not sufficient to produce a perceptible effect in a time of vibration amounting to 1234 seconds, repeated at different hours of the day and night.

In conclusion, there appears reason to presume from the preceding experiments, that the dip itself may be determined by Meyer's needle within a much smaller limit of uncertainty, than has hitherto been the case by needles of the usual construction; as the results are subject only to those errors, which are reducible by repetition; for in the ten experiments which have been submitted to the Society, the greatest difference of any one from the mean does not exceed three minutes; the direct observation may, therefore, be considered as capable of sufficient precision, to justify inferences from repetition, at intervals of short duration; both of the amount, and also of the uniformity, of the changes to which the dip is subject.

II. *Some positions respecting the influence of the Voltaic Battery in obviating the effects of the division of the eighth pair of nerves. Drawn up by* A. P. Wilson Philip, *M. D. F. R. S. Edin. Communicated by* B. C. Brodie, *Esq. F. R. S.*

Read July 5, 1821.

Dr. Philip finding that Mr. Brodie did not think what had been done sufficient to establish certain positions which Dr. Philip had stated, he, for Mr. Brodie's satisfaction, and with his assistance, entered on an investigation, with respect to the results of which, Mr. Brodie agrees with him. They were the following.

In some experiments in which the nerves of the eighth pair were divided in the neck of a rabbit, and the ends not displaced, and the animal was allowed to live some hours, it was found that food swallowed immediately before the division of the nerves, was considerably digested, even when the divided ends of the nerves had retracted to the distance of a quarter of an inch from each other.

In other experiments in which, after the division of the nerves, the divided ends had been turned completely away from each other, little or no perfectly digested food, when the animal was allowed to live some hours, was found in the stomach; and the longer the animal lived, the smaller was the proportion of digested food found in the stomach; the great mass having the appearance of masticated food, which was not sensibly lessened in quantity, however long the

animal lived. In an experiment in which, under such circumstances, the stomach was exposed, from the time of the division of the nerves, to the influence of a voltaic battery sent through the lower portion of the divided nerves, its contents were apparently as much changed as they would have been in the same time in the healthy animal. The change was also of the same kind, the contents of the stomach assuming a dark colour, and those of the pyloric end being more uniform, and of a firmer consistence than those of the central and cardiac portions of the stomach, while the whole contents became less in quantity.

The division of the nerves, in both ways, produced difficulty of breathing and efforts to vomit; neither of which occurred when the stomach and lungs were brought under the influence of a voltaic battery, sent through the lower portion of the divided nerves.*

When, under the foregoing circumstances, the lungs had not been exposed to the voltaic influence, and the animal had been allowed to live for five or six hours, they were found much congested: in the rabbit which had been submitted to this influence, they seemed quite healthy.

* Mr. Brodie was not present till after the death of the animal, but this fact was observed by Mr. Broughton and others.

III. *On some alvine concretions found in the colon of a young man in Lancashire, after death.* By J. G. CHILDREN, *Esq. F. R. S. &c. &c. Communicated by the Society for Promoting Animal Chemistry.*

Read December 13, 1821.

I WAS furnished with the particulars of the following case, through the kindness of JAMES THOMSON, Esq. of Primrose, near Clitheroe.

JOHN CHAMBERS, aged 19, a carpenter at Clitheroe, in Lancashire, was in the habit, during the hot weather of July 1814, of refreshing himself whilst at work, by eating a quantity of unripe plums, of which, at various times, he ate several quarts, and generally swallowed the stones, under the erroneous notion entertained by the lower classes in that neighbourhood, that they would assist the digestion of the fruit. A fellow workman of CHAMBERS, aged 30, pursued the same practice with impunity. Not so the unfortunate subject of this communication, who about Christmas began to complain, but still pursued his occupation and worked, with some interruption, till February 1815, when he applied to Mr. COULTATE, of Clitheroe, for advice, complaining of pain in the abdomen attended with diarrhœa. The abdomen on examination felt tense but not much enlarged, nor had he any feverish symptoms. When in the workshop, he used to lean against the bench, pressing his stomach hard against it, which, he said,

afforded him great relief. Medicines of an astringent nature were first prescribed, which seemed for a time to be of service, but the diarrhœa ere long increased, extreme emaciation took place, and a hard circumscribed tumour was discovered on one side of the abdomen, which, from the thinness of the abdominal parietes, Mr. COULTATE could distinctly feel was an alvine concretion. Clysters were then administered, castor oil given, and the abdomen ordered to be rubbed with oil, under the idea of pushing the concretions forward, but in vain; the patient daily became more and more emaciated, and after about three months attendance he died, on the 6th of May, completely worn out. His appetite was good, or rather almost voracious, even to within a very short time of his death. He always felt himself worse after meals. His stools, especially for some weeks before he expired, were like blood and water. He was confined to his bed for about three weeks before he died. On opening the body, the concretions were found lodged in the arch of the colon, three closely compacted together, rather high up on the left side, the fourth considerably lower, approaching the termination of the colon. The coats of the intestine were much thickened and formed into a sort of pouch, where the concretions lay. The peritoneum was but little inflamed, the other viscera were healthy. The concretion which lay by itself was sawed asunder by Mr. COULTATE, and contained a plum stone in the centre. The body was opened in the presence of the friends of the poor boy, and under circumstances which, unfortunately, prevented Mr. COULTATE from making so minute an examination as he could have wished, and from pressure of business and other unavoidable interruptions, he did not at

under an exhausted receiver, it sunk readily in that fluid. The calculus selected by Sir EVERARD HOME for analysis, was divided by him, and the plum stone nucleus cut through as in the other. The unequal distribution of their component parts, renders it impossible that the results of any two analyses should agree very accurately in the relative proportions of the several ingredients of the concretions; there is no difficulty, however, in ascertaining their nature. They consist of phosphate of lime and ammoniaco-magnesian phosphate, the former in very much the largest proportion; of a large portion of animal matter, principally gelatine; a small portion of resin, and a fine fibrous vegetable substance, from the inner coat enveloping the farina of the oat, which, when the outer husk is removed, is seen to consist of a number of delicate fibres arranged longitudinally round the farina. I did not discover any fatty matter, either by the action of boiling water or a weak alkaline solution, nor any of the substances usually found in urinary calculi, except the phosphates already mentioned.

The method I adopted for the chemical examination of the calculus, was, by submitting it successively, (1) to the action of cold water; (2) of boiling water; (3) of alcohol; (4) of a dilute solution of caustic soda; and (5), of muriatic acid. The cold aqueous infusion exhibited no decided trace of albumen, either on being boiled, or by the test of corrosive sublimate. Sulphate of silver, muriate of baryta, and oxalate of ammonia rendered it slightly turbid. By the second and fourth processes I obtained animal matter, consisting chiefly of gelatine. Its solution in water passed very slowly through the filter whilst cold, more readily when hot. It was not

soluble in alcohol, gave no precipitate with solution of corrosive sublimate, but an abundant white one with infusion of galls. The solution by caustic soda (4), neutralized by weak acetic acid, gave also an abundant precipitate with infusion of galls, and none with corrosive sublimate. It was darker coloured than that obtained by water alone. In the third process, alcohol dissolved a small portion of resin, which water precipitated again perfectly white.

The muriatic acid dissolved the phosphates, and left the vegetable fibre.

To ascertain the relative quantities of the two phosphates, I destroyed the animal and vegetable matter of a fresh portion of the calculus by burning, dissolved the phosphates in muriatic acid, and precipitated them together by ammonia; I then digested the precipitate, whilst moist, in oxalic acid, filtered, and threw down the triple phosphate by ammonia, adding also a little phosphate of soda to secure the precipitation of the whole of the magnesia. The oxalate of lime was then decomposed by heat, its base re-dissolved in muriatic acid, and precipitated by sub-carbonate of ammonia, and the quantity of phosphate of lime inferred from that of the carbonate obtained. The proportions of the several ingredients of the calculus were as follows:

2 } 4 }	Animal matter, chiefly gelatine	25 . 20
3.	Resin - - - -	3 . 90
5 }	Ammoniaco-magnesian phosphate	5 . 16
	Phosphate of lime - - -	45 . 34
6.	Vegetable fibre - - -	20 . 30
		99 . 90

I must not conclude this communication without referring to other similar alvine concretions, which have been at different times met with in those parts of the country where oatmeal is in common use as an article of food among the poorer classes. Dr. MARCET, in his Essay on Calculous Disorders, mentions a concretion which was showed to him by Dr. BOSTOCK, that had been voided by a labouring man in Lancashire; and Dr. MARCET himself examined another given him by Mr. SILVIERA, who had it from Dr. MONRO of Edinburgh. It was in the examination of this calculus, that the true nature of the velvety fibrous substance was ascertained by Dr. WOLLASTON, who, Dr. MARCET says, "found it to consist of extremely mi-"nute vegetable fibres, or short needles, pointed at both ends; "which he immediately conjectured to arise from some kind "of food peculiar to Scotland. For some time, however, he "failed in his attempts to trace this substance to its origin. "But the ingenious Mr. CLIFT, of the College of Surgeons, "to whom the subject was mentioned in conversation, having "put the question, 'whether this fibrous substance might not "'proceed from oats,' Dr. WOLLASTON was induced to ex-"amine the structure of this seed; and the result fully "verified Mr. CLIFT's conjecture." (p. 139).

In Dr. ALEXANDER MUNRO's Morbid Anatomy of the Gullet, mention is made of forty-two alvine concretions collected by the Author's father, which were examined by Dr. THOMSON. Their structure, (with one exception similar in all) is described by Dr. MONRO (p. 32) "as more or less "porous, and somewhat like to dried sponge, and when ex-"amined by the aid of a magnifying glass, seems to be made "up of a number of very small fibres intimately interwoven

"with each other, like those in a hat, or in chamois leather; "and the interstices between the fibres are filled up with "earthy matter." And at p. 34 he adds, "in the centre of "the concretion, a *prune, or a cherry stone*, or a small piece of "bone, or a biliary calculus, is generally found."

Dr. Thomson states the average specific gravity of these concretions to be 1.4. The one I weighed was, as mentioned above, considerably heavier. This may be owing to one containing more of the fibrous substance than the other, or to Dr. Thomson not having employed an exhausted vessel to extricate the air from the pores of the calculus. Dr. Thomson obtained from his analysis, albumen, common salts, phosphate of lime, phosphate of soda and the oat fibres, which he describes as "undoubtedly of a peculiar nature, differing from every animal and vegetable substance hitherto "examined." My results, in most respects, agree with Dr. Thomson's, except that I could not discover any albumen; and on the other hand, the calculi examined by him, do not appear to have contained either the ammoniaco-magnesian phosphate or gelatine.

IV. *On the concentric adjustment of a triple Object-glass.* By WILLIAM HYDE WOLLASTON, *M. D. V. P. R. S.*

Read December 13, 1821.

HAVING in my possession a telescope with a triple object-glass of forty-five inches focus, made by DOLLOND in 1771, I have had a good opportunity of examining the central adjustment of the lenses, and have made trial of a method of correcting that adjustment, which appears not to have been used for that purpose.

When I ventured to take to pieces an instrument that had stood the test of fifty years trial, with uniform approbation of its performance, those who knew the telescope, and who know the difficulty of centering, seemed to consider it an act of rashness which I was likely to regret; but, by the test which I am about to describe, I felt confident that my object-glass was capable of improvement, and I rested my hopes of success on principles that seemed indisputable.

When any bright object is viewed through a glass of this construction, without an eye-glass, there may be observed, at the same time with the refracted image, a series of fainter images, that are formed by two reflections from the different surfaces; and, as the position of each of these images is dependent on the curvatures of that pair of surfaces by which it is formed, they appear at different distances from the object-glass.

Since the number of surfaces is six, the number of binary

combinations of these surfaces is fifteen; and just so many images formed by reflection may be discerned.

It is manifest, that if the glasses be duly adjusted to each other, so that their axes are correctly coincident, then this series of images must be all situated in the same straight line; and conversely, that any defective position may be immediately detected by a derangement of the line of images.

A very distinct view of a part of these appearances, fig. 1. (Pl. I.) is seen by placing the eye close to the object end of the telescope, so as to view the eye-hole illuminated by the flame of a candle set near it. In this position only ten of them are perceived; two beyond the refracted image; four in regular succession nearer to the eye; and four very small ones close together, at a short distance from the object-glass within the tube.

The remaining five images being some inches exterior to the object-glass, cannot be seen till the eye is withdrawn to a greater distance, and are best observed with the assistance of a lens, fig. 2.

Each of the two first named images is formed by a pair of surfaces that are curved in the same direction, fig. 3, of which that which reflects from its concave side is most curved. The next four are also formed by surfaces that are curved in the same direction; but in this case, the convex reflecting surface is more curved than the concave, fig. 4. The four small images arise from pairs of surfaces, that have their convexities opposed to each other, fig. 5; and the five exterior images are owing to those pairs of surfaces which present their concave sides to each other.

In order to explain distinctly the origin of each of these

images, I have, in fig. 7, marked those surfaces which have their concavities toward the tube with large letters, A, B, C, and those curved in the opposite direction with the small letters, *d*, *e*, *f*; by which means, in fig. 8, where the origin of each image is indicated, similarity of source in the several groups will be seen from similarity of notation. The concurrence of a pair of large letters, or a pair of small letters, as BC, or *de*, shows combination of curvatures in the same direction as in figures 3 and 4; unequal letters in the order *d*C, show opposite curvatures, as of fig. 5, giving a negative focus; and the unequal letters, B*f*, in the opposite order, represent the opposite concavities of fig. 6, forming a positive focus beyond the glasses.

The origin of these images was ascertained by giving a small motion to one or the other of the convex glasses. When the outer glass is inclined, fig. 9, all the images dependent on A or *d* are inclined together in the same line *gh*; but it may be remarked, that the image A*d* is not displaced by this motion, as the relative position of the two surfaces A and *d* to each other remains unaltered.

In fig. 10, the inner glass is represented with a similar inclination; and with it all images dependent on C or *f* for their formation, assume an inclined position in the line *ik*.

By lateral motion of the outer glass, it is only the images dependent on A, that are moved; for since the motion takes place in the direction of the curve *d*, which remains in contact with the middle glass, the position of this surface is not altered, fig. 11.

By similar motion of the inner glass, fig. 12, those images only that are owing to *f* are moved, while those from C retain their position.

Any lateral motion given to the central concave has the same effect as moving both the convex glasses together in the opposite direction, and of course the effect is most discernible in the position of an image that is similarly affected by the lateral motion of both. For instance; downward motion of the concave, fig. 13, has the effect of placing a wedge between the two convex glasses from above, so that though each of their inner surfaces still remains in contact with the adjacent concave, their exterior surfaces, A and *f*, are parted from above, and the image A*f*, dependent on them both, is doubly elevated. Hence, the position of A*f* becomes a delicate test of the due centering of the concave glass, and is the best guide in making the final adjustment. In my object-glass, which has been the principal subject of my experiments, this image is very happily situated for this purpose, being so near to the outmost image, A*d*, that the smallest error in their relative position is with the greatest facility detected.

In order that I might have full command of each part, I had a cell constructed of larger dimensions than ordinary, with two pair of adjusting screws, at right angles to each other, applied to the edges of each glass, so that when the images had been first brought into the same vertical plane by means of one set of screws opposed to each other in the horizontal position, the series might next be adjusted to the same horizontal line by the screws placed above and below, at right angles to the former.

In performing these adjustments, there is another pair of images beside A*f*, A*d*, which deserve particular attention, as their motions are independent, and their contiguity renders any variation of their relative position very perceptible. In

figures 11 and 12, the lines *lm*, *no*, mark the irregularities arising from defects in centering of the inner and outer convex glasses, as in fig. 13, the line *p q*, shows the error correspondent to a want of adjustment of the middle concave.

By these guides alone I have now so repeatedly restored my object-glass to correct performance after having removed it from its cell, that I may venture, with considerable confidence, to recommend trial of the method to those who wish to perfect glasses of this construction. The degree of accuracy to be attained will depend upon the smallness and brightness of the light employed.

For the purpose of merely seeing the series of images above described, the entire flame of a candle, not confined by an eye-tube, forms a set of very conspicuous images. To see that the images are not very irregular, an eye-hole of one-fourth or one-fifth of an inch may be used. When the intention is to commence adjustment by candle-light, a single eye-glass of one-tenth or one-fifteenth of an inch focus will be found to give a series of neat images very well suited to the purpose; but, for completing a very nice adjustment, I have found it necessary to employ the light of the sun, and a still smaller lens of one-twentieth or one-thirtieth of an inch focus. With this view, there is no occasion to point the telescope to the sun, for if his light falls even very obliquely on a small eye-glass, the exterior images A *d*, and A*f*, are mere luminous points, so that any error in their relative position is immediately detected.

With this test, as guide for final adjustment, and without farther revision, the telescope on which this method has been tried, is capable of either separating very small and nearly

equal stars as those of 44 Bootis and σ Coronæ, or of exhibiting the minute secondaries of β Orionis and 24 Aquilæ, with as much distinctness as the state of the air will admit. The actual limit to its powers cannot be fully ascertained, excepting under such favourable conditions of the atmosphere as do but rarely occur.

V. *On a new species of Rhinoceros found in the interior of Africa, the skull of which bears a close resemblance to that found in a fossil state in Siberia and other countries.* By Sir Everard Home, *Bart. V. P. R. S.*

Read December 13, 1821.

THE discovery of a new species of any of the larger animals, now that our globe has been so extensively explored, is an object of interest to the naturalist, and might afford sufficient reason for laying this new fact before the Society; but this interest will be much increased, when there is a striking resemblance between the form and appearance of the skull of this animal, now in being, and the skull of one of the same tribe, only met with in a fossil state.

It has been hitherto asserted, as one of the most curious circumstances in the history of the earth, that all the bones that are found in a fossil state, differ from those belonging to animals now in existence; and I believe that this is generally admitted, and that there is no fact upon record, by which it has been absolutely contradicted; but the observations I am about to state respecting this rhinoceros, illustrated by the drawings that accompany them, will go a great way to stagger our belief upon this subject.

The skull of the animal belonging to this new species of rhinoceros, now living in Africa, was brought to this country by Mr. Campbell, one of the Missionaries sent there from

the London Missionary Society, and is deposited in their Museum in the Old Jewry. The following account is taken from the memoranda with which Mr. Campbell very obligingly furnished me.

"The animal was shot about two hundred and fifty, or three hundred miles, up from the westward of De la Goa Bay, six miles west of the city Mashow, and above a thousand miles in nearly a straight direction from the Cape of Good Hope.

"The country from whence the rhinoceros comes, contains no thick woods, or forests, but is covered with separate clumps of trees, like a nobleman's park in England. In travelling, you always appear to be approaching a wood; but as you advance, the trees are discovered to stand at a distance from one another, or rather in little clumps.

"This animal feeds upon grass, and bushes; is not carnivorous; and not gregarious; seldom more than a pair are seen together, or in the vicinity of one another. Mr. Campbell's people wounded another of the same description. When enraged it runs in a direct line, ploughing the ground with its horn. The hide is not welted, is of a dark brown colour, smooth, and without hair."

The skull which Mr. Campbell has brought to England, fortunately has the horns in their natural situation. As the annexed drawing is made upon a scale, and the parts are so clearly exposed, it is hardly necessary to add a verbal description. It will be sufficient to say, that the skull is thirty-six inches long. The long horn, thirty-six inches; the circumference at the base, is twenty-four inches. There are horns

of different lengths in the British Museum, and one forty-two inches; a drawing of which is annexed.

In this skull it will be seen, that the horns differ in many particulars, from those belonging to the other recent species of the rhinoceros. The long one is placed upon the extremity of the nasal bones, with a direction nearly straight forward, and the smaller one so close behind it, as to appear intended for a support to its base. These striking differences would be of little importance, were it not that they make it bear so close a resemblance to the fossil skull from Siberia, as to leave no prominent characteristic mark between them; and were it not that the one is in a fossil state, and the other recent, they would be decided to belong to the same species; for although there is no horn attached to the fossil skull, the surface fitted for it is obviously marked, and no error can be committed, respecting its situation, or direction. The drawing is made from a fossil skull, sent over by the Emperor of Russia to Sir Joseph Banks, and deposited in the British Museum, and compared with another, which came to this country, but was afterwards sent to France. The skull is thirty-three inches long; the largest of the recent rhinoceros in the collection of the Royal College of Surgeons, is two feet.

The drawing of the skull from Siberia is so accurate, as to make a description of its external appearance unnecessary; it is however proper to add, that all the fossil skulls that have been examined, are alike, and three feet long; so that we have now acquired a nearer approach to the form of the skull of the rhinoceros of former ages, in that which is represented in these drawings, and is here described, than has before been obtained. From this fact, so completely

within my own observation, I am led to believe, that although many animals belonging to former ages may be extinct, they are not necessarily so; no change having taken place in our globe, which had destroyed all existing animals, and therefore many of them may be actually in being, although we have not been able to discover them.

When we consider that the course of one of the greatest rivers in Africa, the Niger, has not been traced to its source by any European traveller, we must allow, that great tracts of country in that immense continent remain unexplored; in which those animals, that are not disposed by their nature to submit to the will of man, but, on the contrary, to fly from him, may conceal themselves, by retiring into the wild fastnesses of forests, which for ages to come may never be visited by rational beings. Under these circumstances, we have no right to assume that large animals, although not met with, do not exist.

The following account of the migration of the animals in Africa, is in itself a curious document, and explains in what way particular animals may elude our enquiry at one time, and at another, be brought within our reach.

Mr. Campbell says, "he found that the wild ass, or quagga, migrates in winter from the tropics, to the vicinity of the Malaleveen river, which, though farther to the south, is reported to be warmer than within the tropic of Capricorn, when the sun has retired to the northern hemisphere. He saw bands of two or three hundred, all travelling south, when on his return from the vicinity of the tropic; and various Bushmen, as he proceeded south, enquired if the quaggas were coming. Their stay lasts from two to three months,

which in that part of Africa is called the Bushmen's harvest. The lions who follow them are the chief butchers. During that season, the first thing a Bushman does on awaking, is to look to the heavens to discover vultures hovering at an immense height; under any of them he is sure to find a quagga that had been slain by a lion in the night."

This disposition for migration on large continents, will explain their dispersion into different countries.

It is deserving of remark that the elephant, one of the most powerful and most sagacious of the animal race, has been for ages domesticated (if the expression is admissible), and has learned to have a pride in the ornaments and trappings with which man, for the purpose of pomp and parade, has clothed him. It would appear that the sagacity of this noble animal had taught him, that to live in the bondage and society of men, is better than savage liberty; for when he has returned to a wild state, and remained in it for years, upon meeting with his former guide, immediately on hearing his voice he has returned to his duty. On the other hand, the rhinoceros, although an inhabitant of nearly the same countries, varying equally in species, and met with by men of different nations, in the same degree of frequency, has never been brought to a civilized state; but is at this day so savage and stupid in its nature, that it cannot be tamed.

The elephant, we know from observation, as well as from the size of its brain, particularly the cerebrum, has intellect and memory; but in the rhinoceros, so small is the cavity of the cranium, that in all these respects it must be much inferior to the elephant. The capacity of the cavity of the skull of the male rhinoceros from Sumatra, two feet long, is

to that of the elephant, as thirty-five ounces to one hundred and eighty-two. The length of the skull of the recent rhinoceros brought over by Mr. CAMPBELL, is three feet; and the cavity, although mutilated, shows it not to be larger than the other. In Mr. BROOKS's skeleton of the rhinoceros, five feet six inches high, the skull is only one foot eleven inches. His skeleton of the elephant is six feet six inches; so that Mr. CAMPBELL's rhinoceros must have been of the full size.

The skull of the horse has a capacity which, when compared with that of the rhinoceros, is to the small female of that species, nearly equal.

Skulls of the different species of rhinoceros known to exist, are preserved in the anatomical collections in this country, as well as in France. One species from Sumatra with two horns, one from Africa with two horns, and one with a single horn.

Of all these different species none have been found to possess a common share of intellect; the size of the cavity of the skull in all of them, is nearly the same; and there is no account upon record, of a rhinoceros ever having been tamed, although curiosity alone, would have been a sufficient inducement to have made the attempt, had there been any probability of success.

The following account, of the manners and habits of the Asiatic rhinoceros, clothed in armour, and having the welted hide, I have taken from the young man who was its keeper for three years in the Menagerie at Exeter Change, at the end of which period it died.

It was so savage, that about a month after it came to

Exeter Change, it endeavoured to kill the keeper, and nearly succeeded. It ran at him with the greatest impetuosity; but fortunately the horn passed between his thighs, and threw the keeper on its head: the horn came against a wooden partition, into which the animal had forced it to such a depth, as to be unable for a minute to withdraw it, and during this interval the man escaped.

Its skin, although apparently so hard, is only covered with small scales of the thickness of paper, with the appearance of tortoise shell; at the edges of these, the skin itself is exceedingly sensible, either to the bite of a fly, or the lash of a whip; and the only mode of managing it at all was by means of a short whip. By this discipline the keeper got the management of it, and the animal was brought to know him; but frequently, more especially in the middle of the night, fits of phrenzy came on, and while these lasted, nothing could controul its rage, the rhinoceros running with great swiftness round the den, playing all kinds of antics, making hideous noises, knocking every thing to pieces, disturbing the whole neighbourhood, then all at once becoming quiet. While the fit was on, even the keeper durst not make his approach. The animal fell upon its knees to enable the horn to bear upon any object. It was quick in all its motions: ate ravenously all kinds of vegetables: appearing to have no selection. They fed it on branches of the willow. It possessed little or no memory; dunged in one place, and if not prevented, ate the dung, or spread it over the sides of the wall. Three years confinement made no alteration in its habits.

The account in the Bible of an unicorn not to be tamed, mentioned by Job, bears so great an affinity to this animal,

that there is much reason to believe that it is the same, more especially, as no other animal has ever been described so devoid of intellect. In that age, the short horn might readily be overlooked, as it cannot be considered as an offensive weapon; and the smoothness of the animal's skin would give it a greater resemblance to the horse than to any other animal.

EXPLANATION OF THE PLATES.

Plate II.

Fig. 1. A representation of the skull of the double-horned rhinoceros, brought from Africa by Mr. Campbell, and deposited in the Museum of the Missionary Society, in the Old Jewry: upon a scale of one inch and a half to a foot.

Fig. 2. A representation of the fossil horn of a rhinoceros, preserved in the British Museum: upon a scale of two inches to a foot.

Plate III.

The representation (from a cast) of the fossil skull of the rhinoceros from Siberia, the original of which is in the collection of fossil bones in the Jardin des Plantes, in Paris: upon a scale of three inches to a foot.

VI. *Extract of a Letter from Capt.* Basil Hall, *R. N. F. R. S. to* William Hyde Wollaston, *M. D. V. P. R. S. containing observations of a Comet seen at Valparaiso.*

Read January 10, 1822.

His Majesty's ship Conway,
Valparaiso, May 4, 1821.

My dear Sir,

I have much pleasure in sending you some observations which I have just made upon a comet that has been in sight here for 33 days. It is now almost gone, and I scarcely hope to get another satisfactory observation of its place. I was unfortunately in the interior of the country when it first appeared, so that it was not until the 8th of April that I was able to make any observation with accuracy. Since that day I have determined its place, as you will see, for several other days, and I trust there are data enough to work upon.

ever sincerely your's, &c. &c.

BASIL HALL.

TABLE, showing the Places of the Comet seen at Valparaiso, in April and May

Latitude 33° 2′ 18″,9 S°. Longitude 71° 36′ W.

As observed by Capt. BASIL HALL, Lieut. WILLIAM ROBERTSON, and Mr. HENRY

of His Majesty's Ship Conway.

Date.	Mean Time.	Æ in Time.	Declination. (South.)	Stars with which the Comet was compared. (From Con. des Temps, 1819.	Interval between the Passage of the Comet and the Star. (Sidereal Time.)	Difference of declination. (Comet S°. or N° of ✱.)	REMARKS.
1821. Apr. 8	h. m. s. 7.10.58	h. m. s. 2.34.16	° ′ ″ 7.51.49	10 ρ^3 Eridani (4)	h. m. s. 21.14,5	′ ″ 26.26 N	Clear night. Land br gave the telescope a sl lous motion.
11	6.54.45	2.46.29	7.12. 2	20 τ Orionis (4)	2.22.26,8	9.20 S	Clear still night. Circur vourable.
12	7. 2.30	2.50.14,6	6.58.39	———	2.18.41,7	4. 3 N	Circumstances favourabl
14	6.54.00	2.57.14,9	6.33.45	38 ο Eridani (4)	1. 5.53,8	44.48,6 N	Comet very faint; but tinct in the telescope stances favourable.
17	7.00.29	3. 6.43,1	5.58.11	17 Eridani (4,5)	15. 1,9	16.34,9 S	Comet almost obliterat naked eye by the mo The observations ho good this evening.
18	6.36.52	3. 9.32,1	5.46.17	———	12.13,0	4.40,8 S	All circumstances favour
19	6.34.54	3.12.26,0	5.34. 1	———	9.19,0	7.34,7 N	Comet very faint—weat with a fresh breeze, w tremulous motion to th Circumstances not fav
20	6.28.19	3.15. 8,0	5.23.58	———	6.37,1	17.38,3 N	All circumstances favo though the Comet appe fainter than usual.
21	6.30.10	3.17.46,4	5.13.32,7	———	3.58,6	28. 3,3 N	All circumstances very fa
24	6.49.30	3.25.14,4	4.45.18,3	42 ξ Eridani (3,4)	49.32,6	35.26,3 S	Circumstances all favou met very faint.
29	6.48.36	3.36.29,2	3.57.46,8	———	38.17,8	12. 5,2 N	Circumstances all favou Comet fainter.
May 1	6.31.40	3.40.15,8	3.41.15,9	48 ν Eridani (4)	47. 7,7	2.10,4 N	Circumstances very favou met very distinct.
3	6.29.37	3.44.20,4	3.25.53,1	———	43. 3,1	17.33,2 N	Circumstances favourabl Comet very faint. Th last observation made.

The foregoing right ascensions and declinations were determined by means of a wire micrometer attached to a power of about 80. The comet was always so near the horizon before it became visible, that on no evening during the whole month that it was observed, could its Æ and declination be measured more than once. Unfortunately there were few known stars near the comet, so that it was often necessary to wait a considerable time before any one passed through the field of view. The high range of hills which encircle Valparaiso, also, interfered with these observations, as the comet became hid sooner than it otherwise might have been. During the first week the nucleus was very distinct, and might have been measured by the micrometer; but I was then in the interior of the country, and did not commence observing it till the 8th, when the nucleus had become so indistinct as to render any measurements of this kind uncertain. Lieutenant ROBERTSON of the Conway, assisted by Mr. FOSTER, midshipman of that ship, measured the angular distance of the comet from Aldebaran, Sirius, and Canopus, from its first appearance, until the splendour of the moonlight so far obliterated the comet, as to prevent any good distances being taken. These distances are given here, as they may serve to determine the comet's place, if required, for several days before the micrometrical observations were commenced.

		° ′ ″		° ′ ″		° ′ ″
1st April 7 P.M.	Aldebaran	44.42. 0	2d April 7 P.M.	43.24. 0	3d April 7.10 P.M.	42. 1.40
	Sirius	66.52.40		67. 8.30		65.28.40
	Canopus	68.10.50		67.18.40		66. 5.00
	Barom.	29.91	Barom.	29.86	Barom.	30.
	Ther.	61.	Ther.	64	Ther.	68
7th April 7.15 P.M.	Aldebaran	37.35.22	8th April 7.5 P.M.	36.33.10	9th April 7.2 P.M.	35.35.30
	Sirius	60.46.48		59.45.25		58.49.40
	Canopus	64.11.17		63.45.27		63.20.40
	Barom.	29.97	Barom.	29.86	Barom.	29.94
	Ther.	60	Ther.	60	Ther.	62

(The error of the index has been applied to these distances.)

On its first appearance, the comet was of a dull white colour: the tail seemed to be split, or to have a dark streak between its sides. On the second evening, the tail subtended an angle of 7°, reaching to ρ Ceti: the northern part of the tail was the longest. On the third, the appearance was much the same. It was hid till the seventh by clouds: the tail then appeared shorter, and the nucleus less bright; changes which at the time were ascribed to the interference of the moon's light; but which, I think, must have arisen from the increased distance of the comet. The tail at first was nearly at right angles to the horizon, but at each succeeding night it inclined more to the south. The time of its appearance was always very short, and that time was generally occupied with the adjustment of the micrometer, so that I was not enabled to draw it so frequently as I could have wished; but these few sketches [Pl. IV.] will give some idea of its appearance.

BASIL HALL.

VII. *Elements of Captain* Hall's *Comet. By* J. Brinkley, *D.D. F.R.S. and M.R.I.A. and Andrew's Professor of Astronomy in the University of Dublin. In a Letter addressed to* W. H. Wollaston, *M.D. V.P.R.S.*

Read, January 10, 1822.

Observatory, Trinity College, Dublin,
October 15, 1821.

My dear Sir,

I send you the elements of the comet observed at Valparaiso, the observations of which you were so kind as to send to me.

We are indebted to the science of Captain Hall, for adding this comet to our catalogue.

The observations appear to have been as exact as could have been made even in an established observatory, on a comet only visible so near the horizon, and so far from the meridian, and of which the light was probably faint both from its actual distance from us, and its apparent proximity to the sun.

The comet on the 8th of April was distant nearly 1,41 from the earth, the sun's distance from the earth being unity, and on the 3d of May, when last seen, about 2,64.

This comet is interesting to astronomers on account of its small perihelion distance. In the Catalogue of M. Delambre, out of 116 comets, the orbits of which have been computed, there are only three that pass nearer the sun.

It is interesting also, from the probability that it is the same comet that was observed in 1593, which agrees with this in

its small perihelion distance and great inclination. The inclination was computed by LACAILLE to be 88° nearly. The inclination of this is 106° 44′. The perihelion distance of that = ,089, of this ,093.

This comet would probably have been a very remarkable one by its appearance, had the earth been in a favourable situation.

It may, at first, cause some surprize, that it escaped the watchful observers of Europe when on the north side of the ecliptic, in February and March last, before it passed its perihelion; but on examination it will be found, that in March it was only a few degrees from the sun, and must have been rendered invisible by the superior lustre thereof. It passed from the north to the south side of the plane of the earth's orbit, March 21st, $18^h\ 30^m$, a few hours after it had passed its perihelion; and its elongation from the sun then was only about 4°.

The observations having been made so long after its passage through perihelion, the smallness of its perihelion distance, and some other circumstances, have involved the computation in difficulties not often met with. This induces me to request you will lay before the Royal Society, the method by which I proceeded.

The unusual circumstances relative to this comet, will also appear by referring to a passage in the Preface of M. BURCKHARDT's new table of the Parabolic Movements, &c. in the Conn. des Temps, 1818. It relates to the interval of his table from 1000 to 10,000 days, of which he says, " comme il est probable que cette partie ne servira *jamais* dans la pratique, &c." Now, in the observations after the 18th of April,

the anomalies of this comet exceeded that of 1000 days in the tabular comet.

Besides the elements (B) deduced by the method of which the explanation is given, I had previously deduced another set (A) by a process less certain in its operations. The latter set (A) however, may be thought not much inferior to the other set (B).

I remain, my dear Sir,

your's most truly,

Dr. Wollaston, &c. &c. &c. J. BRINKLEY.

Elements (B.)

		h.	m.	s.
Perihelion distance ,092800				
Time of passage through perihelion Mean Time, Greenwich, March 21st. - - -		13	15	47
	s.	°	′	″
Inclination - -		73	15	48
Node (ascending)	1	18	24	41
Perihelion - -	7	29	6	47
Motion retrograde.				

Elements (A.)

		h.	m.	s.
Perihelion distance ,0894				
Passage through perihelion March 21, - - -		7	3	26
	s.	°	′	″
Inclination - -		74	32	41
Node - - -	1	19	38	17
Perihelion - -	8	0	35	8
Motion retrograde.				

	Observation.	Observation.	Observation.	Error of Elements (B.)		Error of Elements (A.)	
1821.	Mean Time Valparaiso.	Longitude.	Latitude S.	Long.	Lat.	Long.	Lat.
	h. m. s.	° ′ ″	° ′ ″	′ ″	° ″	′ ″	′ ″
April 8	7 10 58	33 27 44,3	21 48 8,0	0 0	0 0	+0 20	0 0
11	6 54 45	36 47 3,4	22 10 15,2	+1 28	+0 29	+0 53	+0 2
12	7 2 30	37 49 5	22 15 26	+1 11	+0 45	+0 26	+0 13
14	6 54 0	39 44 25,9	22 24 26,2	+0 56	+0 7	+0 1	—0 29
17	7 0 29	42 21 57	22 33 9	+0 56	—0 30	0 0	—1 7
18	6 36 52	43 9 14	22 34 7	+1 36	+0 17	+0 45	—0 18
19	6 34 54	43 57 55	22 34 55	+0 1	+0 56	—0 46	+0 24
20	6 28 19	44 42 53	22 36 50	+0 25	+0 9	—0 14	—0 21
21	6 30 10	45 26 57,0	22 37 58,8	+0 36	—0 8	+0 4	—0 34
24	6 49 30	47 31 42,7	22 41 31,3	+1 11	—2 15	+1 6	—2 32
29	6 48 36	50 41 8,7	22 39 37,2	—2 4	—0 38	—1 16	—0 41
May 1	6 31 40	51 44 55,1	22 37 45,6	+2 4	+0 51	+3 13	+0 49
3	6 29 37	52 52 55,1	22 37 42,7	—0 50	+0 15	+0 45	+0 22

Computation of the Elements.

The method used for the first approximation was that of M. LAPLACE.* The observations chosen were those of April 8, 11, and 14. It considerably diminishes the length of the calculation to use only three observations, where it can be done. In the present instance it is not likely, had a greater number of observations been used, that the superior accuracy derived would have compensated for the additional trouble. The result from the three observations, is as exact as would be necessary on common occasions. Here, on account of the small perihelion distance and small motion of the comet in heliocentric longitude between the first and last observations, the method of M. LAPLACE,† "Determination exacte des elemens de l'orbite, &c. &c." is inapplicable; the errors of the motion in heliocentric longitude being greater than the motion itself.

The extension however of that method, as indicated by M. DELAMBRE,‡ might succeed in this instance, but the formidable calculations necessary are sufficient to deter one from the attempt.

The method which I gave in Vol. XIII. p. 189, of the Transactions of the Royal Irish Academy, also, in this case, requires to be extended. It will be necessary to introduce the squares of the variation of the perihelion distance, and of the correction of the time from perihelion.

The manner in which my method is extended to this case will be easily understood from the following detail; and also the change I have made in deriving on common

* Mec. Celes. Tom. i. p. 221, &c. † p. 225. ‡ Astron. Tom. 3. p. 384.

occasions the final equations, by using a formula for the computation of V, different from that given by M. LAPLACE.

Previously to the computation, I re-calculated the observations of April 8, 11, 14, 21, and May 3, using the places of the stars as given by M. PIAZZI. This perhaps was unnecessary. The results were

	Æ	Declination S.
	h. m. s.	° ′ ″
April 8	2 34 15,0	7 51 52
11	2 46 28,0	7 12 4
14	2 57 14,2	6 33 51
21	3 17 46,1	5 13 35
May 3	3 44 19,7	3 25 55

By M. LAPLACE's method, "Determination approchée, &c." the observations of April 8, 11, and 14, give perihelion dist. $(p) = ,0865$, and time of perihelion, March $19^d\ 14^h\ 4^m$.

Let the true perihelion dist. $= p + dp$, and time of perihelion $=$ March $19^d\ 14^h\ 4^m - dt$, so that t being the interval between March $19^d\ 14^h\ 4^m$, and the observation of April 8, the true value of $t = t + dt$.

Let also T $=$ the time in the table of the comet of 109 days, when the anomaly $= \nu$.

$\Delta =$ variation of anomaly in that table in one day at time T, and $r =$ comet's distance from the sun

$$\text{Then } d\nu = \left(\frac{dt}{p^{\frac{3}{2}}} - \frac{3tdp}{p^{\frac{5}{2}}}\right)\Delta \qquad - \qquad - \qquad (1)$$

$$\text{and } (x)\ \frac{d\log r}{\sin 1''} = d\nu \tan \tfrac{1}{2}\nu + \frac{dp}{p\sin 1''} \qquad (2)$$

Conceive the triangle in which S, T, C, represent the sun, earth, and comet respectively, and let P represent the projection of the comet on the plane of the earth's orbit.

In this case, comet's hel. long.=earth's long.—TSP. Also d hel. lat. $(\pi) = x \tan SCT \cot CST \tan \pi$. - - (3)
d hel. long. $(\beta) = -x \cot TSP \tan SCT (\tan CST - \cot CST \tan^2 \pi)$ — (4)

These angles SCT, &c. are found in the course of the calculation.

If β, β' β'' represent three hel. longitudes, and π, π' π'' represent three hel. latitudes, and V, V′ represent the bases of two spherical triangles of which the sides are $90° - \pi$, $90° - \pi'$, and $90° - \pi$, $90 - \pi''$, and the vertical angles $\beta - \beta'$ and $\beta - \beta''$ respectively,

$$\cos V = \cos (\pi - \pi') - 2 \sin^2 \tfrac{1}{2} (\beta - \beta') \cos \pi' \cos \pi \quad (5)$$
$$\cos V = \cos (\pi - \pi'') - 2 \sin^2 \tfrac{1}{2} (\beta - \beta'') \cos \pi'' \cos \pi \quad (6)$$

Taking the observations of April 8, 21, and May 3, and the above values of p and t,

	Anomalies.	Hel. Long.	Hel. Lat. S.
	° ′ ″	S. ° ′ ″	° ′ ″
April 8	139 46 38* $=\nu$	2 5 17 1 $=\beta$	48 55 19 $=\pi$
21	146 38 6 $=\nu'$	2 3 47 40 $=\beta'$	42 43 42 $=\pi'$
May 3	150 12 10 $=\nu''$	2 2 35 30 $=\beta''$	39 22 50 $=\pi''$

If $U = \nu' - \nu$ and $U' = \nu'' - \nu$

The observations of April 8 and April 21, give

$$U = 6° \; 51' \; 28'' \quad V = 6° \; 16' \; 44''$$

Those of April 8, and May 3, give

$$U' = 10° \; 25' \; 32'' \quad V' = 9° \; 44' \; 12''$$

Therefore because $U + dU = V + dV$

and $U' + dU' = V' + dV'$

* The results are put down to seconds; but as only five places in the logarithms were used in this part of the process, they may sometimes err by several seconds.

$$dV - dU = 2084'' \quad - \quad - \quad (8)$$
$$dV' - dU = 2480'' \quad - \quad - \quad (9)$$

By the formulæ above given

$$dv = 2751\, dt - 950060\, dp$$
$$dv' = 1336\, dt - 762080\, dp$$
$$dv'' = 861\, dt - 669980\, dp$$
$$x = 7512\, dt - 209300\, dp$$
$$x' = 4459\, dt - 158300\, dp$$
$$x'' = 3235\, dt - 133450\, dp$$
$$d\pi = -{,}4390\, x \quad .. \quad d\beta = -{,}9345\, x$$
$$d\pi' = -{,}3446\, x' \quad .. \quad d\beta' = -{,}4367\, x'$$
$$d\pi'' = -{,}2954\, x'' \quad .. \quad d\beta'' = -{,}2130\, x''$$

From these values it is necessary to compute dV, dV'.

Let $H = 2 \sin^2 \frac{1}{2} (\beta - \beta') \cos \pi' \cos \pi$

$H = 2 \sin^2 \frac{1}{2} (\beta - \beta'') \cos \pi'' \cos \pi$

then equations (5) and (6) become

$$\cos V = \cos (\pi - \pi') - H \quad - \quad (10)$$
$$\cos V' = \cos (\pi - \pi'') - H' \quad - \quad (11)$$

It is obvious from the smallness of $\beta - \beta'$ and $\beta - \beta''$, and the magnitude of the errors of $U - V$ and $U' - V''$ that the errors of $\beta - \beta'$ and $\beta - \beta''$ may bear a considerable proportion to the quantities themselves, or be even greater. Therefore the coefficients of the differentials dp and dt in dH will be quite incorrect, if the differential of H be computed in the common way.

It is the smallness of $\beta - \beta'$ and $\beta' - \beta''$ in this case that renders M. LAPLACE's method of computing V, V' inconvenient, in consequence of its being necessary to use the tangent of an angle nearly $= 90°$.

But the variations of V and V' may be obtained with sufficient accuracy in the following mann er.

By equation (10) we easily deduce an account of the smallness of H

$$V = \pi - \pi' + \frac{H}{\sin 1'' \sin (\pi - \pi')}$$

and with sufficient exactness

$$V + dV = \pi - \pi' + d\pi - d\pi' + \frac{2 \sin 1'' \cos \pi' \cos \pi (\beta + d\beta - \beta' - d\beta')^2}{\sin (\pi - \pi')}$$

therefore

$$dV = -(d\pi - d\pi') + \pi - \pi' - V + \frac{2 \sin 1'' \cos \pi' \cos \pi (\beta + d\beta - \beta' - d\beta)^2}{\sin (\pi - \pi')}$$

and the same expression serves for dV' changing π', β' and V into π'', β'' and V'.

Hence, substituting the values of $\beta, \beta', \beta'', \pi', \pi'', \pi''$ &c. &c. we obtain

$$dV = -344^* + ,3446x' - 4390x + N(2680 - ,4672x + ,2188x')^2.$$

$$dV' = -703 + ,2954x'' - 4390x + N'(4845 - ,4672x + ,1065x'')^2$$

where log N = 5.63801 and log N' = 5.47328

and finally, equations (8) and (9) give

$$N(2680 - 2534dt + 63150dp)^2 - 364dt - 150649dp = 2428 \quad (12)$$

$$N'(4845 - 3166dt + 83572dp)^2 - 452dt - 227619dp = 3212 \quad (13)$$

From these equations the values of dp and dt may be derived. The indirect solution seems to be the shortest, as we know the value of dp within narrow limits.

The first error of p as deduced from the approximation of LAPLACE, rarely indeed will amount to $\pm$,01.

1. Let us suppose $dp = -$,005. Then equation (12) gives $dt = +4,294$ and $-1,194$. The positive value is too great to be admitted, taking $t = -1,194$, the result from equation (13) is $0 = -468$.

* This number $344'' = 6° 16' 44'' - 6° 11'$ should be strictly $367'' = 6° 16' 44'' - 6° 11' 37''$; but the accidental omission of $37''$ is not of the smallest consequence in the result.

2. $dp = ,000$

$dt = + 4,901$ and $-1,547$, substituting in equation (13)

$t = - 1,547$

$o = - 309$

3. $dp = + ,005$

$dt = + 5,442$ and $- 1,838$

$o = - 137$

the value of dp therefore exceeds $+ ,005$.

The last result shows, that with this value of dp, the errors of V — U and V′ — U′ are reduced to comparatively small quantities, and it may be useless in this first process to endeavour to reduce them still more.

Therefore we may now make $p = ,0865 + ,005 = ,0915$, and $t = 19,9118 - 1,838 = 18,0738$.

With these values we obtain, having corrected the sun's places and comet's observed places for aberration, and made the computations to seven places of figures,

		° ′ ″	s ° ′ ″	° ′ ″
April	8	136 53 51 $= \nu$	2 9 47 32 $= \beta$	50 51 16 $= \pi$
	21	144 50 48 $= \nu'$	2 4 59 14 $= \beta'$	43 38 4 $= \pi'$
May	3	148 48 45 $= \nu''$	2 3 12 5 $= \beta''$	39 56 29 $= \pi''$

$V = 7° 55' 6''$, $V' = 11° 53' 33''$.

On account of the great changes that have taken place in some of these quantities, particularly in the longitudes; it will be better to re-compute $d\nu$, $d\pi$, $d\beta$, &c. &c.

This in common cases would be unnecessary.

The new values are

$$dv = 3299\ dt - 977400\ dp$$
$$dv' = 1510\ dt - 768635\ dp$$
$$dv'' = 947\ dt - 667980\ dp$$
$$x = 8353\ dt - 220500\ dp$$
$$x' = 4768\ dt - 172700\ dp$$
$$x'' = 3390\ dt - 138150\ dp$$
$$d\pi = -,4543x \ .\ d\beta = -1,1466\ x$$
$$d\pi' = -,3599x' \ .\ d\beta' = -\ ,4756\ x'$$
$$d\pi'' = -,3053x'' .\ d\beta'' = -\ ,2252\ x''$$

We may now use the equations (10) and (11) for finding dV and dV'

$$d\text{V} \sin \text{V} = \sin (\pi - \pi')\ (d\pi - d\pi) + \frac{d\text{H}}{\sin 1''}$$

$$d\text{H} = \text{H} \sin 1''(\tan \tfrac{1}{2}\ (\beta - \beta')\ (d\beta - d\beta') - d\pi' \tan \pi' - d\pi \tan \pi)$$

The expressions for dV' and dH' are had by changing H, β' and π' into H', β'' and π''.

Then, by substituting the above values for $d\pi$, $d\pi''$, &c. &c.

$$d\text{V} = -3858\ dt + 80110\ dp$$
$$d\text{V}' = -4888\ dt + 112190\ dp$$

and the equations

$$\text{U} + d\text{U} = \text{V} + d\text{V}$$
$$\text{U}' + d\text{U}' = \text{V}' + d\text{V}$$

become

$$-2069\ dt - 128650\ dp = 111 \quad - \quad (14)$$
$$-2536\ dt - 197230\ dp = 80 \quad - \quad (15)$$

which give

$$dp = +\ ,001425 \text{ and } dt = -\ ,1423$$

Consequently, the new approximate values of p and t are

$$p = ,092925 \text{ and } t = 17,9315$$

Repeating the computation with these values we obtain

	° ′ ″	s ° ′ ″	° ′ ″
April 8	136 22 43 $=v$	2 10 16 43 $=\beta$	51 2 44 $=\pi$
21	144 28 59 $=v'$	2 5 6 34 $=\beta'$	43 43 35 $=\pi'$
May 3	148 30 40 $=v''$	2 3 3 35 $=\beta''$	39 59 56 $=\pi''$

$$V = 8^\circ\, 6'\, 29 \qquad V' = 12^\circ\, 8'\, 7''$$

and thence $U—V = -13''$ and $U'—V' = -11$

Substituting these values on the right hand side of equations (14) and (15), and solving the equations

$$dp = -,0001249 \text{ and } dt = ,014049$$

and the new values of p and t are $p=,092800$, and $t=17,9456$, which are sufficiently exact. From these new values of dp and dt we very easily get from the above values of dv &c. &c.

	° ′ ″	s ° ′ ″	° ′ ″
April 8	136 25 33 $=v$	2 10 13 57 $=\beta$	51 1 39 $=\pi$
21	144 30 56 $=v'$	2 5 5 52 $=\beta'$	43 43 3 $=\pi'$
May 3	148 32 16 $=v''$	2 3 3 20 $=\beta''$	39 59 36 $=\pi''$

$$V=8^\circ\, 5'\, 24'' \qquad V' = 12^\circ\, 6'\, 45''$$

The differences between the first values of β, β' &c. &c. and these correct values, seem deserving of notice.

It remains to find the place of the node, inclination of the orbit, and the place of perihelion.

For this purpose it is very convenient and sufficiently exact* to compute, in the spherical triangle formed by the sides

* We are enabled to use this short process for finding the inclination, in consequence of being able to compute so readily the new values of dV, and dV as well as of $d\pi$, $d\beta$, &c. &c. by substituting in the values above given, the last values of dp, dt. This may be considered as another advantage of this method of correcting the approximate elements of a comet's orbit.

$90-\pi$, $90-\pi''$ and V′, the angle opposite to $90-\pi''$, and also in the spherical triangle formed by $90-\pi$, $90-\pi'$ and V, the angle opposite to $90-\pi'$.

These angles are

$152^\circ\ 38'\ 9''$, which gives the inclination $73^\circ\ 19'\ 50''$

$152\ \ 51\ \ 56$, which gives the inclination $73\ \ 11\ \ 47$

Now, if the orbit were an exact parabola, and the observations perfectly accurate, the above quantities ought, in consequence of the exactness that has been used in the computation, to agree to a second. Hence is seen the unavoidable errors arising from these two causes jointly. It is probable, that the deviation from a parabola has no sensible effect in the present case.

With the mean value $152^\circ\ 45'\ 3''$ we get the place of the node $48^\circ\ 24'\ 41''$, and the place of perihelion $=7^s\ 29^\circ\ 6'\ 47''$.

There evidently appears an irregularity in the three last right ascensions observed April 29, May 1 and 3. It is possible, that this happened in consequence of the difficulty of observing the comet from its faintness: if so, each of the observations may partake of that irregularity, and the inaccuracy of the observation of May 3, may have affected the elements.

This might have been avoided, by combining a greater number of observations in correcting the approximate elements, but the advantage would not, probably, have repaid the additional labour.

The remark on these latter observations, must not be understood to imply, in the smallest degree, a defect in observing; on the contrary, the general exactness of the observations appears highly creditable to the observers, and requires no

allowance for the unavoidable difficulties under which they must have observed.

Note.—The computations relative to the comet observed by Captain HALL, were finished in the middle of October last, and the results immediately sent to London for the purpose of being laid before the Royal Society. The second part of the Transactions for 1821, did not reach me till after the communication had been read at the Royal Society. In that second part, I was much surprised to find the elements of the same comet computed by Mr. RUMKER, from the observations made by Dr. OLBERS, before the passage through perihelion.

Subsequently, the "Conn. des Tems," for 1824 reached me, which contains Observations made at Paris, and Elements by M. NICOLLET; also a notice of the comet having been observed in several places of Europe. It certainly is highly creditable to those observers who discovered, under very difficult circumstances, the comet in its approach to the sun.

By the addition of Captain HALL's observations after the passage through perihelion, we are enabled to obtain very exact elements.

The errors of my Elements, when applied to observations before perihelion, and the errors of Mr. RUMKER's and of M. NICOLLET's Elements, when applied to Captain HALL's observations after perihelion, are considerable:

Therefore, I have farther corrected my Elements, by using Dr. OLBERS' observations of January 30, with those of Captain HALL of April 8, and May 3.

The new elements (C) are

Perihelion distance ,091677		
Time of passage through perihelion - - -	March 21, 11ʰ 11ᵐ 48ˢ	Mean time Greenwich.
Inclination - - - -	73° 34′ 53″	
Node - - - - -	48 42 18	
Place of perihelion - -	239 30 33	

Motion retrograde.

Errors of the Elements (C).

	Error in Longitude.	Error in Latitude.	Observers.
Jan. 21	+0′ 18″	−0′ 12″	M. NICOLLET, Paris.
30	+1 6	+0 13	Dr. OLBERS, Bremen.
Feb. 9	−0 28	−0 44	M. NICOLLET.
Mar. 6	−2 17	−0 57	Dr. OLBERS.
April 8	+0 18	−0 45	Captain HALL, Valparaiso.
20	+0 11	−0 10	Ditto.
May 3	−0 49	+0 12	Ditto.

Between January 21, and May 3, the comet described above 300° about the sun: consequently, as a parabola represents the observations with so much exactness, it follows, that the period of its revolution must be considerable.

J. B.

Observatory, Trinity College, Dublin,
Feb. 25, 1822.

VIII. *On the Electrical phenomena exhibited in vacuo.* By Sir Humphry Davy, *Bart. P. R. S.*

Read December 20, 1821.

The production of heat and light by electrical discharges: the manner in which chemical attractions are produced, destroyed, or modified by changes in the electrical states of bodies: and the late important discovery of the connection of magnetism with electricity, have opened an extensive field of enquiry in physical science, and have rendered investigations concerning the nature of electricity and the laws by which it is governed, and the properties that it communicates to bodies, much more interesting than at any former period of the history of philosophy.

Is electricity a subtile elastic fluid? or are electrical effects merely the exhibition of the attractive powers of the particles of bodies? Are heat and light elements of electricity, or merely the effects of its action? Is magnetism identical with electricity, or an independent agent, put into motion or activity by electricity?—Queries of this kind might be considerably multiplied, and stated in more precise and various forms: the solution of them, it must be allowed, is of the highest importance; and though some persons have undertaken to answer them in the most positive manner, yet there are, I believe, few sagacious reasoners, who think that our present data are sufficient to enable us to decide on such very abstruse and difficult parts of corpuscular philosophy.

It appeared to me an object of considerable moment, and one intimately connected with all these queries, *the relations of electricity to space, as nearly void of matter as it can be made on the surface of the earth;* and, in consequence, I undertook some experiments on the subject.

It is well known to the Fellows of this Society who have considered the subject of electricity, that Mr. WALSH believed that the electrical light was not producible in a perfect torricellian vacuum; and that Mr. MORGAN drew the same inference from his researches; and likewise concluded that such a vacuum prevented the charging of coated glass.—Now it is well known, that in the most perfect vacuum that can be made in the torricellian tube, vapour of mercury, though of extremely small density, exists; I could not help, therefore, entertaining a doubt as to the perfect accuracy of these results, and I resolved not only to examine them experimentally, but likewise, by using a comparatively fixed metal in fusion for making the vacuum, to exclude, as far as was possible, the presence of any volatile matter.

The apparatus that I employed was extremely simple, [Pl. V.] and consisted of a curved glass tube with one leg closed, and longer than the other. In this closed leg a wire of platinum was hermetically cemented, for the purpose of transmitting the electricity: or to ascertain the power of the vacuum to receive a charge, it was coated with foil of tin or platinum. The open end, when the closed leg had been filled with mercury or any other metal, was exhausted either by being placed under the receiver, or connected with the stopcock of an excellent air pump; and in some cases, to ensure

greater accuracy, the exhaustion was made after the tube and apparatus had been filled with hydrogene.*

Operating in this way, it was easy to procure a vacuum either of a large or small size, for the rarefied air or gas could be made to balance a column of fluid metal of any length, from 20 inches to the 20th of an inch, and by using only a small quantity of metal, it could be more easily purged of air.

I shall first mention the results I obtained with quicksilver. I found that by using recently distilled quicksilver in the tubes, and boiling it in vacuo six or seven times from the top to the bottom, and from the bottom to the top, making it vibrate repeatedly by striking it with a small piece of wood, a column was obtained in the tube free from the smallest particle of air; but a phenomenon occurred, in discovering the cause of which I had a great deal of trouble. When I used a short tube of four or five inches long only, I found, that after continued boiling and much agitation of the mercury, though there was no appearance of elastic matter, when the mercury adhered strongly in the upper part of the tube, yet that, after electrization, or even on suffering the mercury to pass slowly back into the closed part, a minute globular space sometimes appeared: I thought at first that this was air, which, though so highly rarefied, as it must have been by the exhaustion, adhered to the mercury; and I endeavoured by long boiling the mercury in an exhausted *double* syphon, and making the vacuum in one of the curves, to prevent entirely the presence of air: but the phenomenon always occurred when there was no strong adhesion of the mercury

* The figure will best explain the form of the apparatus.

to the glass. This, and another circumstance, namely, that when the leg in which the torricellian vacuum was made was 15 or 16 inches long, the phenomenon was very rarely perceptible, and always disappeared when the tube was inverted, and the mercury made to strike the top with some force, led me to conclude that the minute space was really filled with the vapour of mercury; the attraction of the particles of the fluid mercury for each other preventing their actual contact with the glass, except when this contact was forcibly made by mechanical means; and I soon proved that this was the case: for by causing the mercury, when its column was short, to descend into the more perfect from the less perfect vacuum, with more or less velocity, I could make the space more or less, or cause its disappearance altogether, in which last case the cohesion between the mercury and the glass was always extremely strong.

I found that in all cases when the mercurial vacuum was perfect, it was permeable to electricity, and was rendered luminous by either the common spark, or the shock from a Leyden jar, and the coated glass surrounding it became charged; but the degree of intensity of these phenomena depended upon the temperature: when the tube was very hot, the electric light appeared in the vapour of a bright green colour, and of great density; as the temperature diminished, it lost its vividness; and when it was artificially cooled to 20° below zero of FAHRENHEIT, it was so faint as to require considerable darkness to be perceptible.

The charge likewise communicated to the tin or platinum foil was higher the higher the temperature; which, like the other phenomenon, must depend upon the different density of

the vapour of mercury; and at o° FAHRENHEIT it was very feeble indeed.

A very beautiful phenomenon occurred in boiling the mercury in the exhausted tube, which showed the great brilliancy of the electrical light in pure dense vapour of mercury. In the formation and condensation of the globules of mercurial vapour, the electricity produced by the friction of the mercury against the glass, was discharged through the vapour with sparks so bright as to be visible in day light.

In all cases when the minutest quantity of rare air was introduced into the mercurial vacuum, the colour of the light produced by the passage of the electricity changed from green to sea green; and, by increasing the quantity, to blue and purple; and when the temperature was low, the vacuum became a much better conductor.

I tried to get rid of a portion of the mercurial vapour, by using a difficultly fusible amalgam of mercury and tin, which was made to crystallize by cooling in the tube; but the results were precisely the same as when pure mercury was used.

I tried to make a vacuum above the fusible alloy of bismuth; but I found it so liable to oxidate and dirt the tube, that I soon renounced farther attempts of this kind.

On a vacuum above fused tin I made a number of experiments; and by using freshly cut pieces of grain tin, and fusing them in a tube made void after being filled with hydrogene, and by long continued heat and agitation, I had a column of fused tin which appeared entirely free from gas: yet the vacuum made above this, exhibited the same phenomena as the mercurial vacuum. At temperatures below o°, the light was yellow, and of the palest phosphorescent kind,

requiring almost absolute darkness to be perceived; and it was not perceptibly increased by heat.

I made two experiments on electrical and magnetic repulsions and attractions in the mercurial vacuum, by attaching to the platinum wire two fine wires in one case of platinum, in the other of steel, terminated by minute spherules of the same metals: I found that they repelled each other when the wire was electrified in the most perfect mercurial vacuum, as they would have done in usual cases; and the steel globules were as obedient to the magnet as in the air; which last result it was easy to anticipate.

In some of the first of these experiments, I used a wire for connecting the metal with the stop-cock; but latterly, the rarefied air or gas was the only chain of communication; and this circumstance enabled me to ascertain that the feebleness of the light in the more perfect vacuum was not owing merely to a smaller quantity of electricity passing through it, for the same discharge which produced a faint green light in the upper part of the tube, produced a bright purple light in the lower part, and a strong spark in the atmosphere.

The boiling point of pure olive oil is not much below that of mercury; and the butter or chloride of antimony (antimonane) boils at about 388° Fahrenheit. I tried both these substances in the vacuum, and found, as might be expected, that the light produced by the electricity passing through the vapour of the chloride was much more brilliant than that produced by it in passing through the vapour of the oil; and in the last it was more brilliant than in the vapour of mercury at common temperatures: the lights were of different colours, being of a pure white in the vapour of the chloride,

and of a red, inclined to purple, in that of the oil; and in both cases permanent elastic fluid was produced by its transmission.

The law of the diminution of the density of vapours by diminution of temperature, has not been accurately ascertained; but I have no doubt, from the experiments of Mr. DALTON, and some I have made myself, that it is represented by a geometrical progression; the decrements of temperature being in arithmetical progression; and in three pure fluids that I operated upon,* the ratio seemed nearly uniform for the same number of degrees below the boiling point; and (taking intervals of 20 degrees of temperature) .369416. Upon this datum, and considering the boiling points of mercury to be 600°, that of oil 540°, that of the chloride of antimony 340°, and that of tin 5000° all above 52°, and the elastic force of vapour of water at this temperature to be equal to raise by its pressure about .45 parts of an inch of mercury; the relative strengths of vapour will be, for mercury 000015615, for oil 0016819, for chloride of antimony 01692, and for tin 37015, preceded by 48 zeros.†

It is not known whether the vapour from solids follows a similar law of progression as that from fluids, and these numbers are only given to show how minute the quantity of matter must be in vapours where its effects are distinct upon electrical phenomena; and how much more minute it must be in the case of mercury artificially cooled; and almost beyond imagination so in vapours from substances requiring very elevated temperatures for their ebullition.

* Water, chloride of phosphorus, and alcohol or carburet of sulphur.

† I am obliged to CHARLES BABBAGE, Esq. F.R.S. for these calculations.

I made some comparative experiments to ascertain whether below the freezing point of water, the diminution of the temperature of the torricellian vacuum diminished its power of transmitting electricity, or of being rendered luminous by it. To about 20° this appeared to be the case; but between 20° above and 20° degrees below zero, the lowest temperature I could produce by pounded ice and muriate of lime, it seemed stationary; and as well as I could determine, the electrical phenomena were nearly of the same intensity as those produced in the vacuum above tin.

Unless the electrical machine was very active, no light was visible during the transmission of the electricity; but that this transmission took place, was evident from the luminous appearance of the rarefied air in the other parts of the syphon, and from the diminution of the repulsion of the ball of the quadrant electrometer attached to the prime conductor. When the machine was in great activity, there was a pale phosphorescent light above, and a spark on the mercury below, and brilliant light in the common vacuum. A Leyden jar *weakly* charged could not be made to transmit its electricity by explosion through the cooled torricellian vacuum, but this electricity was slowly dissipated through it; and when *strongly* charged, the spark passed through nearly as much space as in common air, and with a light visible in the shade. At all temperatures below 200°, the mercurial vacuum was a much worse conductor than highly rarefied air: and when the tube containing it was included in the exhausted receiver, its temperature being about 50°, the spark passed through a distance six times greater in the Boylean than in the mercurial vacuum.

It is evident from these general results that the light (and

probably the heat) generated in electrical discharges depends *principally* on some properties or substances belonging to the ponderable matter through which it passes; but they prove likewise that space, where there is no appreciable quantity of this matter, is capable of exhibiting electrical phenomena: and, under this point of view, they are favourable to the idea of the phænomena of electricity being produced by a highly subtile fluid or fluids, of which the particles are repulsive, with respect to each other, and attractive of the particles of other matter. On such an abstruse question, however, there can be no demonstrative evidence. It may be assumed, as in the hypothesis of HOOKE, HUYGENS, and EULER, that an ethereal matter, susceptible of electrical affections, fills all space; or that the positive and negative electrical states, may increase the force of vapour from the substances in which they exist; and there is a fact in favour of this last idea which I have often witnessed—when the voltaic discharge is made in the Boylean vacuum, either from platinum or charcoal, in contact with mercury, the discharging surfaces require to be brought very near in the first instance; but the electricity may be afterwards made to pass to considerable distances through the vapour generated from the mercury or charcoal by its agency;—and when two surfaces of highly fixed metal, such as platinum or iron are used, the discharge will pass only through a very small distance, and cannot be permanently kept up.

The circumstance, that the intensity of the electrical light in the mercurial vacuum diminishes as it is cooled to a certain point, when the vapour must be of almost infinitely small density, and is then stationary, seems strongly opposed to the idea, that it is owing to any *permanent* vapour emitted

constantly by the mercury. The results with tin must be regarded as more equivocal; because as this substance cannot be boiled in vacuo, it may be always suspected to have emitted a small quantity of the rare air or gas to which it has been exposed; yet, supposing this circumstance, such gas must be at least as highly expanded as the vapour from cooled mercury, and can hardly be supposed capable of affording the dense light, which the passage of the electricity of the charged Leyden phial through the vacuum produces.

When the intense heat produced by electricity is considered, and the strong attractive powers of differently electrified surfaces, and the rapidity of the changes of state, it does not seem at all improbable, that the superficial particles of bodies, which, when detached by the repulsive power of heat, form vapour, may be likewise detached by electrical powers, and that they may produce luminous appearances in a vacuum, free from all other matter by the annihilation of their opposite electrical states.

In common cases of electrical action, the quantity of the heat generated by the annihilation of the different electrical states depends, as I stated in my last communication to the Society, upon the nature of the matter on which it acts; and in cases when electrical sparks are taken in fluids, vapour or gas is always generated; and in elastic fluids, the intensity of the light is always greater, the denser the medium. The luminous appearances therefore, it is evident from all the statements, must be considered as secondary; whilst the uniform exertions of electrical attractions and repulsions, under all circumstances, in rare and dense media and in vacuo, and with respect to solids, fluids, and gases, point them out (whe-

ther they be specific affections of a subtile imponderable fluid, or peculiar properties of matter) as primary and invariable electrical phenomena.

I have mentioned in the last page the suspicion, that melted tin may contain air. I shall conclude this paper by stating the grounds of this suspicion, and noticing a circumstance which appears to be of considerable importance, both in relation to the construction of barometers and thermometers, and to the analysis of gaseous bodies. Recently distilled mercury that has been afterwards boiled and cooled in the atmosphere, and which presents a perfectly smooth surface in a barometer tube, emits air when strongly heated in vacuo, and that in quantities sufficient to cover the whole interior of the tube with globules; and on keeping the stop-cock of one of the tubes used in the experiments on the mercurial vacuum open for some hours, it was found that the lower stratum of mercury had imbibed air, for when heated in vacuo, it emitted it distinctly from a space of a quarter of an inch of the column; smaller quantities were disengaged from the next part of the column; and its production ceased at about an inch high in the tube. There is great reason to believe, that this air exists in mercury in the same invisible state as in water, that is distributed through its pores; and the fact shows the necessity of long boiling the mercury in barometer and thermometer tubes, and the propriety of exposing as small a surface of the mercury as possible to the air. It may explain, likewise, the difference of the heights of the mercury in different barometers; and seems to indicate the propriety of reboiling the mercury in these instruments after a certain lapse of time.

EXPLANATION OF THE PLATE.

Plate V.

A. The tube, of the usual diameter.

B. The wire for communicating electricity.

E. A small cylinder of metallic foil, to place as a cap on tubes not having the wire B, to make a coated surface.

C. The surface of the quicksilver, or fused tin.

D. The part of the tube to be exhausted by the stop-cock F, after being filled by means of the same stop-cock, when necessary, with hydrogene.

G. The moveable tube connected with the air-pump.

It is evident, that by introducing more mercury, the leg D may be filled with mercury, and the stop-cock closed upon it, so as to leave only a torricellian vacuum in the tube, in which the mercury may be boiled. I have found that the experiment tried in this way, offers no difference of result.

IX. *Croonian Lecture. On the anatomical structure of the Eye; illustrated by microscopical drawings, executed by* F. Bauer, *Esq. By Sir* Everard Home, *Bart. V. P. R. S.*

Read November 15, 1821.

Having found an extraordinary advantage from Mr. Bauer's microscopical observations, when applied to anatomical investigations of other organs of animals, I requested him to give me his assistance in the examination of the different parts of the eye in the human species, quadrupeds, and birds.

In the first place, I wished him to ascertain whether the marsupium in the bird's eye is muscular; as I had advanced such an opinion in the Lecture for 1795. After the most careful examination, he has decided that it is not; but is a fine vascular membrane, as represented in the annexed drawing, which Dr. Young had long considered it The real structure of the marsupium being thus completely established, I was led to inquire what parts, contained within the globe of the eye, are possessed of muscular fibres. Mr. Bauer, on examining the ciliary processes, found that the anterior layer is made up of about 80 processes, lying directly behind the iris, and with it firmly attached at the base to the choroid and sclerotic coat: these are membranous, very vascular, and the surface next the lens concave; that next the iris, convex. They are distinctly shown in the annexed drawings.

Between these membranous processes there are bundles of muscular fibres of $\frac{25}{100}$ of an inch in length, which I believe have not before been described: they originate all round from the capsule of the vitreous humor, pass forward over the edge of the lens, are attached firmly to its capsule and there terminate, as is seen in the drawings. They are unconnected with the ciliary processes, or iris. In the human eye, and that of the quadruped, they form bundles with intermediate spaces. In the bird, they are nearly one continued layer of muscular fibres. The choroid coat, which may be said to terminate anteriorly at the ciliary processes, is membranous in its structure; so far similar to the marsupium of the bird when unfolded; but in the choroid coat, Mr. BAUER has discovered lymphatic vessels, one on each side of every principal artery, not before ascertained, although there could be no doubt of their existence; and the arteries, as will be seen in the drawings, have not the usual direction, but run parallel to one another, totally unlike those in the marsupium.

In the quadrupeds with the tapetum lucidum, the nigrum pigmentum is principally deposited between the sclerotic and choroid coat. In the human eye and that of the bird, between the choroid coat and retina, a thin pellucid covering being interposed between it and the expansion of the optic nerve; the marsupium has a similar covering between the nigrum pigmentum and the vitreous humor. The colour of the nigrum pigmentum differs in intensity according to the colour of the hair; and when the animal is quite white, appears to be altogether wanting. That the marsupial membrane secretes the nigrum pigmentum, there can be no doubt; and for that purpose it is very abundantly supplied with large

arteries. That the choroid in the human eye, as well as that of the bird does the same, is in some measure proved by the fine injection thrown into the arteries of that membrane escaping from their termination, so as to form a layer of injection behind the retina without the smallest appearance of extravasation.

The membrane between the nigrum pigmentum and retina, has been described by Dr. Jacob, of Dublin, and his account of it published in the Philosophical Transactions. The nigrum pigmentum appears to be nothing more than the colouring matter of the red globules, rendered black in the act of separation from the arteries; it is also deposited upon the surface of the ciliary processes and iris, by the arteries with which they are loaded, and covered by a pellucid membrane.

In the horse, the arteries of the ciliary processes are very large; and I am led to believe that the extraordinary disease that animal is liable to in India, of having two species of worms (the strongylus armatus and filiaris papillosa) found alive in the aqueous humor, is produced by the ova or young worms escaping from the terminations of those arteries; more especially as Mr. Hodgson, in his engravings of diseased arteries, has figured the strongylus armatus adhering to the inner membrane of the superior mesenteric artery of the horse, showing that in that animal it gets into the circulation.

The iris is fixed at its origin to the annular ligament; is divisible into two layers; the posterior, muscular; the fibres radiating towards the pupil, at which part there is a regular sphincter muscle; the anterior, membranous.

My friend Mr. Maunoir, of Geneva, is, I believe, the first person who made out this structure, and gave an engraving of

it. I have much pleasure in stating, that what he represented in the quadruped, corresponds with Mr. BAUER's drawings from the human eye, made before Mr. MAUNOIR's Treatise on the artificial pupil was shown to him. The capsule of the crystalline lens is made up of two hemispheres of different texture, in which the lens is completely enclosed; the anterior portion is more dense than the other. The posterior is so thin, as to appear a continuation of the capsule of the vitreous humor, but from its curling up when cut, it must partake of the same nature as the anterior portion. The vitreous humor consists of a very delicate gelatinous substance, exceedingly elastic, abundantly supplied with branches of vessels. Arteries are sometimes met with carrying red blood, and in a degree capable of being injected.

The fibres of the lens have the appearance of hairs like those formed in spun glass.

The situation of the marsupium is shown both in the eagle and goose, and the difference of its radius of curvature at the bottom of the eye on its two sides, is as $\frac{7}{40}$ to $\frac{8}{40}$th of an inch.

EXPLANATION OF THE PLATES.

PLATE VI.

Fig. 1. Front view of the human eye, extracted from its orbit; natural size.

Fig. 2. Side view of the same; natural size.

Fig. 3. Vertical section of the same; magnified three diameters.

Fig. 4. External side view of the human eye, the sclerotic coat and cornea being removed, showing the bundles of vessels which arise from the sclerotic coat, and go towards the ciliary ligament; magnified three diameters

Fig. 5. Internal view of a vertical section of the preceding figure, bringing to view the inside of the iris with the nigrum pigmentum, the ciliary processes, the radiated and pigmented circle in the anterior arch of the eye, the retina, and a portion of the medullary substance of the optic nerve and the choroid membrane; magnified three diameters.

Fig. 6. Side view of the vitreous humor and crystalline lens, taken out of the eye, to show the impression left on it by the pigment of the radiated circle, and ciliary processes; magnified three diameters.

Fig. 7. External view of the iris, consisting entirely of a plexus of vessels; magnified three diameters.

Fig. 8. Internal view of the iris, consisting of bundles of muscular fibres; those next to the pupil being orbicular, and the exterior radiated; magnified three diameters.

PLATE VII.

Fig. 1. Internal view of a small portion of the iris of the human eye, the pigmentum nigrum being removed to show, *a*, *a*, the orbicular, and *b*, *b*, the radiated muscle; magnified ten diameters.

Fig. 2. Internal view of a portion of the membranous part of the human eye, consisting of *a*, *a*, a small portion of the iris with the pigment; *b*, *b*, the ciliary processes in their natural state; *c*, *c*, a portion of the radiated circle in the anterior arch of the eye, its pigment appears to have been disturbed by the vitreous humor being forcibly removed; *d*, *d*, the commencement of the retina, and *e*, *e*, the edge of the choroid membrane; magnified ten diameters.

Fig. 3. External view of the same; *a*, *a*, the iris, *b*, *b*, two of the ciliary processes, *c*, *c*, the seam or ligament by which all

these parts are joined to the sclerotic coat; *d, d,* the ciliary ligament, and *e, e,* the choroid membrane; magnified ten diameters.

Fig. 4. The same as the preceding figure, with the iris removed, to give a back view of the ciliary processes at *a, a*; *b, b,* shows the seam or ligament by which these parts are joined to the sclerotic coat; *c, c,* the ciliary ligament, and *d, d,* the choroid membrane; magnified ten diameters.

Fig. 5. An equally sized portion of the anterior part of the capsule of the crystalline lens, *a, a,* and of the capsule of the vitreous humor, *b, c, d,* exactly lying under, and corresponding with the preceding figures 2, 3, and 4, and showing the bundles of fibres, *b, b,* by which the capsule of the crystalline lens is connected with the capsule of the vitreous humor; and how the pigment from the ciliary processes, *b, b,* and from the radiated circle, *c, c,* is impressed on it; magnified ten diameters.

Fig. 6. A couple of the fibrous bundles, with the nigrum pigmentum nearly removed; magnified twenty diameters.

Fig. 7. Front view of one of the ciliary processes laid open, and the pigmentum removed, to show the plexus of vessels in its membranous wings, from which the nigrum pigmentum seems to be secreted; magnified twenty diameters.

Fig. 8. A back view of the same; magnified twenty diameters.

Fig. 9. An external view of a small portion of the choroid membrane; magnified ten diameters.

PLATE VIII.

Fig. 1. A front view of an injected bullock's eye; natural size.

Fig. 2. Side view of the same; natural size.

Fig. 3. Vertical section of the same; natural size.

Fig. 4. Side view of the vitreous humor and the crystalline lens; natural size.

Fig. 5. Outside view of the iris; magnified two diameters.

Fig. 6. Inside view of the same, the nigrum pigmentum being entirely removed; magnified two diameters.

Fig. 7. Inside view of a small portion of the iris, the nigrum pigmentum entirely removed; magnified eight diameters.

Fig. 8. A portion of the membranous coat of the eye, consisting of a small portion of the iris, the nigrum pigmentum partially removed, the injected ciliary processes, a portion of the choroid membrane slightly injected, and a very small portion of the retina adhering to it; magnified six diameters.

Fig. 9. One of the ciliary processes, finely injected; magnified ten diameters.

Fig. 10. A portion of the capsule of the vitreous humor, with its muscular fibres connecting it with the anterior part of the capsule of the crystalline lens; magnified six diameters.

PLATE IX.

Fig. 1. Side view of a dissected goose's head; natural size.

Fig. 2. Front view of the same; natural size.

Fig. 3. Front view of the left eye of the goose; natural size.

Fig. 4. Side view of the same; natural size.

Fig. 5. Vertical section of the left eye of the goose; magnified three diameters.

Fig. 6. The vitreous humor and crystalline lens of the same eye; magnified three diameters.

Fig. 7. Transverse section, or the posterior hemisphere of the same left eye; magnified three diameters.

Fig. 8. Represents at A, a portion of the anterior part of the capsule of the crystalline lens; at B, a very small portion of the posterior part of the capsule of the crystalline lens; and at C, a portion of the capsule or membrane of the vitreous humor, with the muscular fibres which connect it with the capsule of the lens; magnified ten diameters.

PLATE X.

Fig. 1. The marsupium of an injected goose's eye, in its natural state; magnified eight diameters.

Fig. 2. The same marsupium unravelled; at A, in the natural state, and at B, injected; magnified eight diameters.

Fig. 3. Front view of one of the ciliary processes, injected and expanded; magnified twenty diameters.

Fig. 4. Back view of the same; magnified twenty diameters.

Fig. 5. External view of the injected iris; magnified three diameters.

Fig. 6. Internal view of the same, the nigrum pigmentum removed; magnified three diameters.

Fig. 7. Internal view of a portion of the anterior hemisphere of the injected goose's eye, consisting of a portion of the iris, with the nigrum pigmentum, the ciliary processes in their natural position; the radiated circle, and a very small

portion of the injected choroid membrane; magnified ten diameters.

Fig. 8. External view of the same, showing the injected iris, the seam by which these parts are connected with the sclerotic coat, some of the bundles of vessels which arise from the sclerotic coat and enter under or about the connecting seam, part of the injected radiated circle, and a very small portion of the injected choroid membrane; magnified ten diameters.

Fig. 9. Internal view of a portion of the iris, the nigrum pigmentum being entirely removed; magnified ten diameters.

Fig. 10. Horizontal section of the crystalline lens of the goose's eye; magnified three diameters.

Fig. 11. Vertical section of the same; magnified three diameters.

Plate XI.

Fig. 1. Represents, at A, a small portion of the retina, and, at B, a very small portion of that delicate membrane which lies between the retina and the nigrum pigmentum, and which is described by Dr. Jacob, in the Philosophical Transactions of 1819; magnified fifty diameters.

Fig. 2. A very small portion of a single lamella of the crystalline lens of the goose's eye, partly unravelled; magnified one hundred diameters.

Fig. 3. Some separated fibres of the same; magnified four hundred diameters.

Fig. 4. A $\frac{1}{100}$th part of a square inch of the choroid membrane of the injected goose's eye; the portion at A, is in the

natural state, and that at B, is injected; magnified forty diameters.

Fig. 5 A very small portion of the same; at A, in the natural state, and at B, injected; magnified two hundred diameters.

PLATE XII.

Fig. 1. Side view of a dissected eagle's head; natural size.

Fig. 2. Front view of the same; natural size.

X. *A Letter from* John Pond, *Esq. Astronomer Royal, to Sir* Humphry Davy, *Bart. President of the Royal Society, relative to a derangement in the Mural Circle at the Royal Observatory.*

Read November 22, 1821.

My Dear Sir,

The interest which the Royal Society has always taken in every thing relating to this Observatory, and to which may be attributed its present prosperous condition, will, I trust, render unnecessary any apology for this communication.

I wish to make known to astronomers, as soon as possible, a derangement that has for some time past existed in the mural circle, and of which I have not, till lately, been able to ascertain the cause with certainty.

This derangement began, I believe, about the autumn of the year 1819; the position of the telescope was then changed; and from that time the error has been gradually increasing till last summer, when the cause was distinctly ascertained, and the proper remedy applied.

In the Preface to the Greenwich Observations for the year 1820, now printing, I shall have an opportunity of stating the amount of this error, and the correction which should be applied to the observations made within the two last years. At present it will be sufficient to mention, in as few words as possible, the cause of this error.

Those who are acquainted with the construction of the

Greenwich Mural Circle, are aware, that though the telescope may be applied to every part of the circle, yet, when fixed for observation, the principle of the instrument requires that the tube, especially at its extremities, should be so firmly fixed to the circle as to form one piece with it: to accomplish this, connecting braces are attached at each end of the telescope.—It now appears that these braces have, in progress of time, become insecure, owing to the screws which fastened them having given way. The effect of this will be, to permit the ends to bend from the centre instead of retaining, as they ought to do, an invariable position with respect to the circle. Under these circumstances, when the telescope is directed to the zenith, the position may be considered as free from error; but when the instrument is moved either towards the north or south horizon, should either extremity bend more than the other, an error will take place, and will increase from the zenith towards the horizon, but in what exact proportion, remains to be determined by future observations.

The cause of this error being thus ascertained, Mr. Troughton has applied additional braces to connect the telescope with the circle, sufficiently strong, I should conceive, to prevent the possibility of such an accident for the future.

This alteration has already produced such an improvement in the observations, as prove sufficiently that the source of error has not been mistaken. Of the published observations, only those made in the three last months of the year 1819 are affected by this error, and that in so very small a degree, as must have entirely escaped notice, had it not afterwards increased.

During the year 1820 the error increased; but did not, I believe, in the distance from the pole to the equator amount to two seconds; at altitudes lower than Sirius, and at the altitude of the sun at the winter solstice, the error may have been greater than two seconds, but did not exceed four.

But after the month of February 1821, the error rapidly increased; and this ultimately led to a discovery of the cause.

My present object in this letter, is simply to state these circumstances to the Society. I shall defer a more detailed account of the nature of this derangement, and of its effects, till I shall be enabled, by a sufficient number of observations made with the instrument in its improved state, to ascertain, with some degree of certainty, at what period the derangement took place, and what corrections are required to be applied, till the instrument was restored to its perfect state.

I have the honor to be,

My Dear Sir,

with the highest regard,

your most obedient humble Servant,

JOHN POND.

Royal Observatory,
Nov. 21, 1821.

XI. *On the finite extent of the Atmosphere.* By William Hyde Wollaston, *M. D. V. P. R. S.*

Read January 17, 1822.

The passage of Venus very near the sun in superior conjunction in the month of May last, having presented an opportunity of examining whether any appearance of a solar atmosphere could be discerned, I am in hopes that the result of my endeavours, together with the views which induced me to undertake the inquiry, may be found deserving of a place in the Philosophical Transactions.

If we attempt to estimate the probable height to which the earth's atmosphere extends, no phenomenon caused by its refractive power in directions at which we can view it, or by reflection from vapours that are suspended in it, will enable us to decide this question.

From the law of its elasticity, which prevails within certain limits, we know the degrees of rarity corresponding to different elevations from the earth's surface; and if we admit that air has been rarefied so as to sustain only $\frac{1}{100}$ of an inch barometrical pressure, and that this measure has afforded a true estimate of its rarity, we should infer from the law, that it extends to the height of forty miles, with properties yet unimpaired by extreme rarefaction. Beyond this limit we are left to conjectures founded on the supposed divisibility of matter: and if this be infinite, so also must be the extent of

our atmosphere. For, if the density be throughout as the compressing force, then must a stratum of given thickness at every height be compressed by a superincumbent atmosphere, bearing a constant ratio to its own weight, whatever be its distance from the earth. But if air consist of any ultimate particles no longer divisible, then must expansion of the medium composed of them cease at that distance, where the force of gravity downwards upon a single particle is equal to the resistance arising from the repulsive force of the medium.

On the latter supposition of limited divisibility, the atmosphere which surrounds us will be conceived to be a medium of finite extent, and may be peculiar to our planet, since its properties would afford no ground to presume that similar matter exists in any other planet. But if we adopt the hypothesis of unlimited expansion, we must conceive the same kind of matter to pervade all space, where it would not be in equilibrio, unless the sun, the moon, and all the planets possess their respective shares of it condensed around them, in degrees dependent on the force of their respective attractions, excepting in those instances where the tendency to accumulate may be counteracted by the interference of other kinds of matter, or of other powers of which we have no experience, and concerning which we cannot expect to reason correctly.

Now, though we have not the means of ascertaining the extent of our own atmosphere, those of other planetary bodies are nevertheless objects for astronomical investigation; and it may be deserving of consideration, whether, in any instance, a deficiency of such matter can be proved, and whe-

ther, from this source, any conclusive argument can be drawn in favour of ultimate atoms of matter in general. For, since the law of definite proportions discovered by chemists is the same for all kinds of matter, whether solid, or fluid, or elastic, if it can be ascertained that any one body consists of particles no longer divisible, we then can scarcely doubt that all other bodies are similarly constituted; and we may without hesitation conclude that those equivalent quantities, which we have learned to appreciate by proportionate numbers, do really express the relative weights of elementary atoms, the ultimate objects of chemical research.

These reflections were originally suggested by hearing an opinion hazarded without due consideration, that the non-existence of perceptible atmosphere around the moon, might be regarded as conclusive against the indefinite divisibility of matter. There was, however, an oversight in this inference, as the quantity of such matter, which the moon would retain around her, could not possibly be perceived by the utmost power of any instruments hitherto invented for astronomical purposes. For, since the density of an atmosphere of infinite divisibility at her surface would depend on the force of her gravitation at that point, it would not be greater than that of our atmosphere is where the earth's attraction is equal to that of the moon at her surface. At this height, which by a simple computation is about 5000 miles from the earth's surface, we obviously can have no perceptible atmosphere, and consequently, should not expect to discern an atmosphere of similar rarity around the moon.

It is manifestly in the opposite direction that we are to look for information. We should examine first that body

which has the greatest power, and see whether even there the non-appearance of those phenomena which might be expected from such an atmosphere, will warrant the inference that our own is confined to this one planet by the limit set to its divisibility.

By converse of the same rule which gives an estimate of extreme rarity at the moon's surface, we may form a conception at what distance round the sun refraction from such a cause should be perceived. If we calculate at what apparent distance from the body of the sun his force is equal to that of gravity at the surface of the earth, it is there that his power would be sufficient to accumulate (from an infinitely divisible medium filling all space) an atmosphere* fully equal in density to our own, and consequently producing a refraction of more than one degree, in the passage of rays obliquely through it.

If the mass of the sun be considered as 330.000 times that of the earth, the distance at which his force is equal to gravity will be $\sqrt{330.000}$, or about 575 times the earth's radius; and if his radius be 111,5 times that of the earth, then this distance will be $\frac{575}{111.5}$ or 5.15 times the sun's radius; and $15' \, 49'' \times 5{,}15 = 1^\circ \, 21' \, 29''$, will be the apparent distance from the sun's centre on the 23d of May, when the following observations were made.

What deduction should be allowed for the effect of heat,

* Such an atmosphere would, in fact, be of greater density on account of the far greater extent of the medium affected by the solar attraction, although of extreme rarity; but the addition derived from this source, may be disregarded in the present estimate, without prejudice to the argument, which will not be found to turn upon any minute difference.

it may be time to consider when we have learned the amount of apparent refraction at some given distance; and we may then begin to conjecture, whether heat can counteract the increase of density that would occur in the approach of only $\frac{1}{10}$ of a second towards his centre.*

As I had not any instrument in my possession that I considered properly adapted for the purpose, I requested the assistance of several astronomical friends in watching the progress of Venus to the sun for some days preceding superior conjunction, and in recovering sight of her afterwards. But neither the Astronomer Royal at Greenwich, nor Professor BRINKLEY of Dublin, nor Mr. SOUTH, with the admirable instruments they possess, were able to make any observation within the time required, not being furnished with the peculiar means adapted to this inquiry.

Captain KATER, however, who entered fully into my views, and engaged in the prosecution of them with all the ardour necessary for success, by using a reflecting telescope, was able to furnish me with a valuable set of observations, $3\frac{1}{2}$ days preceding conjunction, which, together with those in which I had the good fortune to succeed at nearly an equal interval subsequent to the passage, afford data quite sufficient to show, that no refraction is perceptible at the period

* If we attempt to reason upon what would be the progressive condensation of such an atmosphere downwards towards the surface of the sun, we are soon stopped by the limit of our experience as to the degree of condensation of which the atmosphere is susceptible. If we could suppose the common law of condensation to extend as far as forty-six miles in depth, the density corresponding to it, would be about equal to that of quicksilver, from whence a refraction would occur exceeding all bounds of reasonable calculation. A space of forty-six miles at the distance of the sun from us, would subtend about $\frac{1}{10}$ of a second.

of our observations; and these come far within the specific distance above estimated.

A selection from the series given to me by Captain Kater is contained in the following table:

		Diff. R. A.	Diff. calc. from N. Alm.
	h. m. s.	m. s.	m. s.
May 18	2 40 25	4 25.6	. . .
	21 30 50	3 43.1	. . .
	23 27 58	3 38.8	. . .
19	0 0 0	. . .	3 37
		Diff. Decl.	
May 18	2 44 33	45 56	
	23 19 40	40 57	
19	. . .	. . .	40 36

It is evident, that in these observations the differences between the observed and calculated places of the planet, are not such as to indicate a refraction that can be relied on.

My own observations were very few in number, and not to be compared to the former in precision; but they are necessary to supply a deficiency when Captain Kater was at a distance from his instruments, and could make no observation.

On the 26th, between XI. 20 and XI. 30 I had three comparative observations, the best of which gave me the passage of Venus $3^m\ 55^s$ after the sun. The mean of two others being $3^m\ 49^s$. I consider the result as on the 25th, $23^h\ 24^m$. Diff. R. A. $3^m\ 52^s$.

The nearest second to be inferred from the Nautical Almanac for this time being $3^m\ 53^s$ after the sun, it is evident that no perceptible refraction occurred at this time.

From the observations of Captain Kater, no retardation of

the motion of Venus can be perceived in her progress toward the sun, as would occur from increasing refraction; and by comparison of her motion in the interval between his last observation and my own, with her change of place for the same interval given in the Nautical Almanac, there seems no ground whatever to suppose that her apparent position has been in the least affected by refraction through a solar atmosphere, although the distance at the time of Captain KATER's last observation was but 65′ 50″ from the sun's centre, and at the time of my own only 53′ 15″.

Although these distances appear small, I find that Venus has been seen at a still less distance by Mons. VIDAL of Montpellier in 1805.* On the 30th of May, he observed Venus 3^{m} 16^{s} after the sun, when their difference of declination was not more than 1′, so that her distance from the centre was about 46 minutes of space. Since his observations also accord with the calculated places of Venus, they might have superseded the necessity of fresh observations, if I had been duly aware of the inference to be drawn from them.

The same skilful observer has also recorded an observation of Mercury on the 31st of March of the same year, when he was seen at about 65′ from the sun's centre.

If I were to describe the little telescope with which my observations were made, without taking due care to explain the precautions adopted, and the grounds of their efficacy, it might perhaps be scarcely credible, that with an object glass less than one inch in aperture, having a focal length of only seven inches, I could discern an object not to be seen by telescopes of four and five inches aperture. We know, how-

* Conn. des Tems. 1808.

ever, that this small aperture is abundantly sufficient for viewing Venus at a distance from the sun; and, since the principal obstruction to seeing her nearer (when the atmosphere is clear), arises from the glare of false light upon the object-glass, the success of the observation depends entirely on having an effectual screen for the whole object-glass, which is obviously far more easy to accomplish in the smaller telescope.

Since the screen which I employed was about six feet distant from my object-glass, a similar protection for an aperture of five inches would have required to be at the distance of thirty feet, to obviate equally the interference of the sun's light at the same period; but this is a provision with which regular observatories are not furnished for the common purposes of astronomy.

As I hope at some future time to avail myself of a larger aperture for such observations, without the necessity of mounting a more distant screen, it may be desirable that I should suggest to others the means by which this may be effected, if they think the question of a solar atmosphere worthy of farther investigation.

If an object-glass of four inches aperture be covered, so as to expose only a vertical slit of its surface one inch in width, the surface of glass to be so used is about five times as large as the circular aperture one inch in diameter, and yet will be as completely shaded by a vertical screen at any given distance: and an interval of only five feet, might allow a star or planet to be seen within a degree of the sun's disc.

When the sun and planet have the same declination, the vertical position of the slit is manifestly the most advantage-

ous that could be chosen on the meridian; but, for the purpose of seeing to the greatest advantage when the line of the centres is inclined to the horizon, it would be requisite to have the power of turning the slit and screen together at right angles to any line of direction of the centres.

The only fixed star sufficiently near to the ecliptic, and bright enough to give any prospect of its being seen near the sun, is Regulus, which passes between the 20th and 21st of August; but I have not yet had an opportunity of ascertaining within what distance from the sun this star can be discerned.

In the foregoing remarks, I have perhaps dwelt more upon the consideration of the solar atmosphere, than may seem necessary to those who have considered the common phenomena observable in the occultations of Jupiter's satellites by the body of the planet. Their approach, instead of being retarded by refraction, is regular, till they appear in actual contact; showing that there is not that extent of atmosphere which Jupiter should attract to himself from an infinitely divisible medium filling space.

Since the mass of Jupiter is full 309 times that of the earth, the distance at which his attraction is equal to gravity must be as $\sqrt{309}$, or about 17.6 times the earth's radius. And since his diameter is nearly eleven times greater than that of the earth, $\frac{17.6}{11} = 1.6$ times his own radius will be the distance from his centre, at which an atmosphere equal to our own should occasion a refraction exceeding one degree. To the fourth satellite this distance would subtend an angle of about $3^\circ 37'$, so that an increase of density to $3\frac{1}{2}$ times our common

atmosphere, would be more than sufficient to render the fourth satellite visible to us when behind the centre of the planet, and consequently to make it appear on both (or all) sides at the same time.

The space of about six miles in depth, within which this increase of density would take place, according to known laws of barometric pressure, would not subtend to our eye so much as $\frac{1}{300}$ of a second, a quantity not to be regarded in an estimate, where so much latitude has been allowed for all imaginable sources of error.

Now though, with reference to the solar atmosphere, some degree of doubt may be entertained in consequence of the possible effects of heat which cannot be appreciated, it is evident that no error from this source can be apprehended in regard to Jupiter; and as this planet certainly has not its due share of an infinitely divisible atmosphere, the universal prevalence of such a medium cannot be maintained; while, on the contrary, all the phenomena accord entirely with the supposition that the earth's atmosphere is of finite extent, limited by the weight of ultimate atoms of definite magnitude no longer divisible by repulsion of their parts.

XII. *On the expansion in a series of the attraction of a Spheroid.* By James Ivory, *M. A. F. R. S.*

Read January 17, 1822.

The purpose of this paper is to make some observations on the developement of the attractions of spheroids, and on the differential equation that takes place at their surface.

1. The whole of this doctrine depends on one fundamental proposition. Let $f(\theta, \varphi)$ denote any function of the sines and cosines of the variable arcs θ and φ; and put $\mu = \cos\theta$: then the given function may be developed in a series, viz.

$$f(\theta, \varphi) = Q^{\circ} + Q^{(1)} + Q^{(2)} \ldots + Q^{(i)} \ldots \&c.$$

every term of which will separately satisfy this equation in partial fluxions, viz.

$$\frac{d \cdot \left\{(1-\mu^2)\frac{dQ^{(i)}}{d\mu}\right\}}{d\mu} + \frac{1}{1-\mu^2} \cdot \frac{dd\,Q^{(i)}}{d\varphi^2} + i\,(i+1)\,Q^{(i)} = 0.$$

Now in one case there is no difficulty. Whenever $f(\theta, \varphi)$ stands for a rational and integral function of μ, $\sqrt{1-\mu^2} \cdot \sin\varphi$, $\sqrt{1-\mu^2} \cdot \cos\varphi$; or of three rectangular co-ordinates of a point in the surface of a sphere; the proposition is clear. In this case the same combinations of the variable quantities are found in the terms of the series and in the given function; and by employing the method of indeterminate coefficients, the two expressions may be made to coincide. The inquiry is therefore reduced to examine the nature of the developement when $f(\theta, \varphi)$ is not such a function as has been men-

tioned. One thing is indisputable. Since the terms of the developement contain no quantities, except such as arise from combining three rectangular co-ordinates, it follows, that when $f(\theta, \varphi)$ is not explicitly a function of μ, $\sqrt{1-\mu^2}$. sin φ, $\sqrt{1-\mu^2}$. cos φ, it must be considered as transformed into such a function. Algebraically speaking, the transformation is no doubt always possible; but there may be danger that, by proceeding in this way, we fall upon expressions which are not proper representatives of the given function; which are symbolical merely, and which cannot be safely employed in the investigation of truth.

In order to fix the imagination, and to avoid every sort of uncertainty and obscurity, I shall take a particular, although a very extensive case of the general expression. I shall suppose that $f(\theta, \varphi)$, or more shortly y, denotes a rational and integral and finite function of the four quantities, sin θ, cos θ, sin φ, cos φ. We may then substitute for the powers and products of sin φ and cos φ, their values in the sines and cosines of the multiples of the arc; by which means we shall obtain,

$$y = M^{(0)} + M^{(1)} \cos\varphi + M^{(2)} \cos 2\varphi + \&c.;$$
$$+ N^{(1)} \sin\varphi + N^{(2)} \sin 2\varphi$$

the symbols $M^{(0)}$, $M^{(1)}$, $N^{(1)}$ &c. standing for rational and integral functions of cos θ and sin θ, or of μ and $\sqrt{1-\mu^2}$. Again, every even power of $\sqrt{1-\mu^2}$ is an integral function of μ; and every odd power is equal to a similar function multiplied by $\sqrt{1-\mu^2}$: the value of y will therefore be thus expressed,

$$y = \left(F(\mu) + \sqrt{1-\mu^2} . f(\mu)\right) + \left(F^{(1)}(\mu) + \sqrt{1-\mu^2} . f^{(1)}(\mu)\right)\cos\varphi + \&c.$$
$$+ \left(G^{(1)}(\mu) + \sqrt{1-\mu^2} . g^{(1)}(\mu)\right)\sin\varphi$$

all the functional quantities being integral expressions of μ. The general term of this expression is

$$\left\{ F^{(i)}_{(\mu)} + \sqrt{1-\mu^2}.f^{(i)}_{(\mu)} \right\} \cos i\varphi$$
$$+\left\{ G^{(i)}_{(\mu)} + \sqrt{1-\mu^2}.g^{(i)}_{(\mu)} \right\} \sin i\varphi;$$

and by multiplying by $\frac{(1-\mu^2)^{\frac{i}{2}}}{(1-\mu^2)^{\frac{i}{2}}}$, or 1, it will become,

$$\left\{ \frac{F^{(i)}(\mu)}{(1-\mu^2)^{\frac{i}{2}}} + \frac{f^{(i)}(\mu)}{(1-\mu^2)^{\frac{i-1}{2}}} \right\} (1-\mu^2)^{\frac{i}{2}} \cos i\varphi$$
$$+\left\{ \frac{G^{(i)}(\mu)}{(1-\mu^2)^{\frac{i}{2}}} + \frac{g^{(i)}(\mu)}{(1-\mu^2)^{\frac{i-1}{2}}} \right\} (1-\mu^2)^{\frac{i}{2}} \sin i\varphi;$$

and finally, by expanding the denominators, we get,

$$M^{(i)}(1-\mu^2)^{\frac{(i)}{2}} \cos i\varphi + N^{(i)}(1-\mu^2)^{\frac{i}{2}} \sin i\varphi;$$

the symbols $M^{(i)}$ and $N^{(i)}$ denoting rational series of the powers of μ. By performing these operations in all the terms containing φ, and likewise by expanding the radical $\sqrt{1-\mu^2}$ in the first term, the value of y will be thus expressed;

$$y = M^{(0)} + M^{(1)}(1-\mu^2)^{\frac{1}{2}} \cos\varphi + M^{(2)}(1-\mu^2)^{\frac{2}{2}} \cos 2\varphi + \&c.$$
$$+ N^{(1)}(1-\mu^2)^{\frac{1}{2}} \sin\varphi + N^{(2)}(1-\mu^2)^{\frac{2}{2}} \sin 2\varphi$$

all the functional symbols standing for series of the powers of μ. The given expression now consists entirely of combinations of the quantities μ, $\sqrt{1-\mu^2}\,.\cos\varphi$, $\sqrt{1-\mu^2}\,.\sin\varphi$; that is, it is a function of three rectangular co-ordinates.

The same end might have been accomplished by a shorter and more simple process, which will apply to every function of two variable arcs.

Put $u = \sqrt{1-\mu^2} . \sin \phi$, $z = \sqrt{1-\mu^2} . \cos \phi$; then

$$\cos \theta = \mu$$
$$\sin \theta = \sqrt{1-\mu^2}$$
$$\sin \phi = \frac{u}{\sqrt{1-\mu^2}}$$
$$\cos \phi = \frac{z}{\sqrt{1-\mu^2}}:$$

and by substituting these values and expanding the radical quantities wherever they occur, y will be transformed into a function of μ, u, z, or of three rectangular co-ordinates. The former process has been chosen under the idea, that it exhibits more clearly the quantities which are in a manner extraneous to the function, and are introduced merely for the purpose of making it put on a certain form.

Having now reduced y to a function of three rectangular co-ordinates, the developement in question will be obtained by the method of indeterminate coefficients already mentioned. Nothing more is necessary than to form the quantities $Q^{(0)}$, $Q^{(1)}$, $Q^{(2)}$, &c., giving to each the most general expression, and leaving the coefficients indeterminate; then the sum of these terms will contain the same combinations of μ, $\sqrt{1-\mu^2} . \sin \phi$, $\sqrt{1-\mu^2} . \cos \phi$, that y does; and by making the two expressions coincide, we shall obtain,*

$$y = Q^{(0)} + Q^{(1)} + Q^{(2)} \ldots + Q^{(i)} . \text{\&c.}$$

Now let us go back to the original expression of y. By following the process described in the second chapter of the third book of the *Mecanique Celeste*, a similar developement of that quantity will be obtained. But it is proved, in the same chapter, that a given function cannot be developed two diffe-

* Mecanique Celeste, Tom. ii. p. 42.

rent ways, in a series of terms that satisfy the general equation in partial fluxions:* and from this it appears, that the developement obtained by the algebraic operations described above, is identical with the developement found by LAPLACE's method.

There is another way of expressing the terms of the developement of y, namely, by definite integrals. But upon this head there is no difficulty, when the possibility of the developement is allowed. The question is not concerning the properties of a series of quantities that satisfy the general equation of partial fluxions; but whether the developement can be admitted in all cases as a fit instrument for the investigation of truth.

Since it has been shown that the developement of y, obtained by the procedure in the second chapter of the third book of the *Mecanique Celeste*, is the same with the like developement found by immediately transforming the given expression into a function of three rectangular co-ordinates; it is just to say of the former method, that in reality it is nothing more than a particular way of effecting such a transformation, while at the same time it gives to the transformed quantity a certain arrangement. In the process we have followed, there is no need to employ the differential equation that takes place at the surface of the spheroid; and by thus going more directly to the foundations of the method, we can discern more clearly what goes on under the cover of many complicated analytical operations. There is an essential distinction with regard to the developement to be observed between the two cases when the given expression y is explicitly a function

* Mec. Cel. Tom. II. pp. 32, and 33.

of three rectangular co-ordinates, and when it must be made to assume the form of such a function by a transformation.

When y is an explicit function of three rectangular co-ordinates, these things are true:

1st. The developement contains no quantities except what are found in y. The two expressions are entirely equivalent, being in reality the same quantities differently arranged.

2dly. When y is a finite expression, the developement will consist of a finite number of terms; and when y is a converging series of an infinite number of terms, the developement will be a like converging series. For, in the case of a converging series, we may approach to the value of y as near as we please, by taking in a determinate number of the terms; and the developement of this portion of the series will likewise consist of a finite number of terms.

On the other hand, the properties that demand attention are very different, when we suppose that y is not explicitly a function of three rectangular co-ordinates.

1st. The developement will always contain an infinite number of terms.

2dly. In most instances; and, more particularly, in the very general example that has been considered above; the terms of the developement will involve an infinite number of quantities which do not appear in the original function, and which are introduced merely in order to give the developement its peculiar form.

The original function and the whole infinite series of which the developement consists, may be represented by this equation, viz.

$$y = y + M - M;$$

the right hand side, which stands in place of the developement, containing all the quantities in y, and besides an infinite number of other quantities, denoted by $+$ M and $-$ M, which have opposite signs, and destroy one another. But this mutual destruction of the extraneous quantities will take place only when the totality of the series, comprehending its infinite number of terms, is taken into account. Any determinate number of terms of the developement will contain quantities not to be found in y; and, for aught that appears, the difference may have any given amount. No finite part of the developement can therefore properly be said to represent the proposed function. If we take a separate term, as $Q^{(i)}$, it is not extravagant to say that it may have nothing in common with the original expression. Nevertheless such a term is a necessary part of the series, in order to balance quantities that occur with opposite signs in other terms.

For the sake of illustration, suppose a spheroid of revolution determined by this equation, viz.

$$r = a \left\{ 1 + e\mu\sqrt{1-\mu^2} \right\};$$

e being a small coefficient, and μ the cosine of an arc reckoned from one of the poles of revolution. In such a spheroid the greatest diameters will make an angle of $45°$ with the equator, resembling the figure which the planet Saturn was, some years ago, supposed to have. In order to develope the variable part of the radius of the spheroid, that is $\mu\sqrt{1-\mu^2}$, in a series of quantities that satisfy the equation in partial fluxions, it is first necessary to expand $\sqrt{1-\mu^2}$; and the number of terms of the developement will therefore be infinite.

To the preceding equation let there be added the term

$f\mu \sin \varphi$; φ denoting the variable angle that the circle on which the distance from the pole is reckoned, makes with a circle given by position; and the nature of the spheroid will be thus expressed, viz.

$$r = a\left\{1 + e\left(\mu\sqrt{1-\mu^2} + f\mu \sin \varphi\right)\right\}.$$

In this case the quantity to be developed must be put under this form, viz.

$$\mu\sqrt{1-\mu^2} + \frac{f\mu}{\sqrt{1-\mu^2}}\sqrt{1-\mu^2} \sin \varphi:$$

and the developement will not only consist of an infinite number of terms, but these terms will contain an infinite number of quantities which arise from the expansion of the radical in the denominator, and which are not to be found in the original function.

There is therefore a real distinction to be made between the two cases when y is an explicit function of three rectangular co-ordinates, and when it is not. A method of calculation which is clear, exact and elegant, when it is confined to the first case, becomes clouded with obscurity, if not merely symbolical, when it is extended to the other case. To say the least, there are certainly great difficulties which are not explained; and if there be any geometers who hesitate, and have doubts, they are not without their excuse, and ought not to be entirely condemned.

2. We come next to consider the differential equation that takes place at the surface of a spheroid. Of this equation, three demonstrations have been published; one, in the second chapter of the third book of the *Mecanique Celeste*;* another by the same author, not precisely the same with the former,

* Tom. II. p. 28.

but similar to it, in a memoir read to the Academy of Sciences in 1818; and a third by M. Poisson, in an interesting and profound memoir on the distribution of heat in solid bodies. The two last demonstrations are fundamentally the same; but as M. Poisson has stated the reasoning more fully, and fixed the sense of the proof more precisely, I wish to refer to his memoir. One observation it is proper to make, which is, that in the integration by which the differential equation is proved, the function expressing the thickness of the molecule is considered as invariable, or is treated as a constant quantity. It is essential to attend to this remark, which in reality affords the clue necessary to unravel what is mysterious in this investigation.

In order to acquire a distinct notion of the meaning of the differential equation in the sense in which it is demonstrated, conceive the surface of the earth, perfectly smooth and spherical, to be covered with circles, we shall say, of a thousand yards radius each. The circles may either touch one another and cover the whole surface of the earth; or they may cover it partially only, and with any interruptions that can be imagined: conceive also that a mass of matter, or molecule, is placed within every circle; the thicknesses of the molecules being entirely arbitrary, and subject to no law of variation or restriction whatever, excepting that they are quantities of inconsiderable magnitude when compared with the radius of the sphere. These things being supposed, the differential equation will be separately true of every one of the molecules.

Let now the whole surface of the earth, or any portion of it, be covered with molecules, the thickness varying according

to a particular law, or being expressed by some function of the arcs that determine the position of a molecule: the differential equation will be true of all these molecules. But it will be equally true of them, if the thickness be entirely arbitrary, and subject to no law of variation. It is true, in reality, of each molecule taken in an insulated manner, and because its thickness is some determinate quantity. How then can such an equation be a fit means of proving that the thickness varies in one way rather than another, or that it comes under a particular developement?

To consider this matter more particularly, let $y = f(\theta, \phi)$ denote, as before, the thickness of the molecule; and put,

$$y' = f(\theta', \phi'),$$

$$\gamma = \cos\theta \cos\theta' + \sin\theta \sin\theta' \cos(\phi - \phi'),$$

$$\rho = \sqrt{\gamma^2 - 2ra \cdot \gamma + a^2},$$

a being less than r: then π denoting the semi-circumference to the radius 1, the differential equation at the surface is what the following formula becomes in the particular case of $r = a$, viz.

$$y = \frac{r}{4\pi} \cdot \iint \left\{ \frac{1}{\rho} + 2a \frac{d \cdot \frac{1}{\rho}}{da} \right\} y' \sin\theta' \, d\theta' \, d\phi';$$

or, by substituting the value of ρ,

$$y = \frac{4}{r\pi} \iint \frac{(r^2 - a^2)\, y' \sin\theta' \, d\theta' \, d\phi'}{\left(r^2 - 2ra\gamma + a^2\right)^{\frac{3}{2}}}; \qquad \text{(A)}$$

the integral being taken between the limits $\theta' = 0$, $\phi' = 0$ and $\theta' = \pi$, $\phi' = 2\pi$, and making $a = r$, after the integration.

Now, when $r = a$, $r^2 - a^2 = 0$; and in this case all the elements of the integral are evanescent, unless in the particular circumstances when the denominator is evanescent, or when

$\gamma=1$; which can happen only when $\theta'=\theta$, and $\varphi'=\varphi$. If therefore we suppose r^2-a^2 to be indefinitely small,* the whole value of the integral will be obtained by extending the integration to such values of θ' and φ' as differ indefinitely little from θ and φ. But when θ' and φ' differ indefinitely little from θ and φ, y' or $f(\theta', \varphi')$, will differ indefinitely little from y, or $f(\theta, \varphi)$: and hence it follows that, in the whole extent of the integral, we may consider y' as constant and equal to y. We have therefore to prove the truth of this formula, viz.

$$y=\frac{yr}{4\pi}\cdot\iint\frac{(r^2-a^2)\sin\theta'\,d\theta'\,d\varphi'}{\left(r^2-2ra\gamma+a^2\right)^{\frac{3}{2}}},$$

between the limits $\theta'=o$, $\varphi'=o$, and $\theta'=\pi$, $\varphi'=2\pi$, in the particular case of $a=r$.

The two arcs θ, θ' and the arc of which γ is the cosine, are the three sides of a spherical triangle; $\varphi-\varphi'$ is the angle opposite to the last arc; and if $\psi-\psi'$ denote the angle opposite to θ', the element of the spherical surface will be equally expressed by $\sin\theta'\,d\theta'\,d\varphi'$, or $d\varphi' d.\cos\theta$, and by $d\psi' d\gamma$: wherefore we have to integrate this formula, viz.

$$y=\frac{yr}{4\pi}\iint\frac{(r^2-a^2)\,.\,d\psi' d\gamma}{\left(r^2-2ra\gamma+a^2\right)^{\frac{3}{2}}}.$$

Now integrate between the limits $\psi'=o$ and $\psi'=2\pi$: then

$$y=\frac{yr}{2}\int\frac{(r^2-a^2)\,d\gamma}{\left(r^2-2ra\gamma+a^2\right)^{\frac{3}{2}}}:$$

integrate again, and

$$y=\frac{y}{2a}\cdot\frac{r^2-a^2}{\sqrt{r^2-2ra\gamma+a^2}};$$

then take this integral between the limits $\gamma=+1$, and $\gamma=-1$; and

$$y=\frac{y}{2a}\cdot\left\{\frac{r^2-a^2}{r-a}-\frac{r^2-a^2}{r+a}\right\},$$

which equation is true when we make $r = a$. This analysis is equivalent to the demonstration of M. Poisson. Whatever may be thought of the reasoning, it cannot be denied that, in both processes, y' is in fact treated as a constant quantity. The equation is true of each individual molecule taken separately, and merely because its thickness has some determinate value. Such a demonstration cannot therefore be employed to prove that the thickness of a series of molecules covering the surface of a sphere, or a part of that surface, follows a certain law of variation, or comes under a particular developement.

But a legitimate process of reasoning requires that, in the formula (A), while a represents any determinate quantity less than r, y' be considered as a function of the variable quantities $\sin\theta'$, $\cos\theta'$, $\sin\varphi'$, $\cos\varphi'$; and likewise that the integration be extended to the whole surface of the sphere, or to that part of it covered with the related molecules; after which the true value of the formula will be obtained by making $a = r$. The whole system of molecules being comprehended in the result, we may thence deduce, by a reverse process, the law according to which their thickness must vary, in order to produce that result. Now the integration here spoken of, cannot be executed, unless in the case when y' is explicitly a function of three rectangular co-ordinates. It is therefore only in this case that the differential equation can be considered as rigorously proved; and it is remarkable, that, when we seek from that equation the developement of y', it always comes out in a function of three rectangular co-ordinates.

When y' is not explicitly a function of three rectangular

co-ordinates, the formula (A) cannot be integrated. And, perhaps, what is now said, is alone sufficient to show that, in this case, some modification takes place, which it were desirable to have fully explained. On attempting to transform y' into an expression containing γ, $\sqrt{1-\gamma^2}$, $\sin\psi'$, $\cos\psi'$ in place of $\cos\theta'$, $\sin\theta'$, $\sin\varphi'$, $\cos\varphi'$, the powers of $\sqrt{1-\gamma^2}$ make their appearance as divisors; and hence it is to be feared that the integral will be infinite at the limits; which circumstance would make it impossible to conclude with certainty what the value sought will become in the particular case of $a=r$. But it would be of no utility to seek a strict demonstration of the differential equation: because in reality the method, when it is extended to all functions of two variable arcs, is independent of that equation, being derived from this proposition, that every such expression is either explicitly a function of three rectangular co-ordinates, or may be transformed into one. The developement in question may always be found, as has been shown, by the rules of algebra; and the differential equation is wanted neither for proving the possibility of the developement, nor for calculating its terms. But in this plainer way of considering the matter, it appears that the developement does not represent the given expression y', when that expression is not an explicit function of three rectangular co-ordinates, in the same sense that it does when it is such a function. There is, therefore, a difficulty left unexplained; and we may be permitted to doubt, whether so important a part of the celestial mechanics, as that regarding the figure of the planets, rests, with sufficient evidence, on the doctrine laid down concerning the generality of the developement.

The observations that have been made, relate only to the foundation of the method in the *Mecanique Celeste,* and do not touch upon the general scope of the analysis, which is deserving of every praise. It is natural to think, that the theory of the figure of the planets would be placed on a firmer basis, if it were deduced directly from the general principles of the case, than when it is made to depend on a nice, and somewhat uncertain point of analysis. To hazard a conjecture suggested in the course of writing this paper, the theory will probably be found to hinge on this proposition, that a spheroid, whether homogeneous or heterogeneous, cannot be in equilibrium by means of a rotatory motion about an axis, and the joint effect of the attraction of its own particles, and of the other bodies of the system, unless its radius be a function of three rectangular co-ordinates. If this proposition were clearly and rigorously demonstrated, the analysis of LAPLACE, in changing the ground on which it is built, would require little or no alteration in other respects.

Dec. 22, 1821.

XIII. *On the late extraordinary depression of the Barometer.* By Luke Howard, *Esq. F. R. S.*

Read January 24, 1822.

The quicksilver having fallen to a lower point in the barometer in the course of last month than any person, probably, remembers to have seen it at, in the neighbourhood of London, a short account of the circumstances may, perhaps, not be unacceptable to the Royal Society.

On the evening of December 24, I found the barometer at 28.20 in. the wind being moderate at S. E., with steady rain, the temp. without, at 8 p. m. 45°. Water boiled freely at 210°. Finding the depression still to continue, I took a portable barometer, on Sir H. Englefield's construction, and having ascertained its height to be, at 11 p. m., 27.96 in., I set it up in my chamber on the first floor. At 5 a. m. the 25th, this instrument gave 27.83 in., and I have reason to think it did not go much lower: the rain had ceased early in the night, and it had become somewhat star-light, with a calm air, and hazy cirrostrati above: soon after five, however, the wind rose again, bringing some rain, apparently from N. W., but there was no tempest that I had opportunity to observe, though it might have blown hard during the few hours I slept. The pencil of my clock barometer travelled precisely to two-tenths below the bottom of the scale, having made a continuous downward sweep of nearly an inch and four-tenths in 24 hours: it appears to have turned to rise

abruptly, and by 8 a. m. was again on the point of passing 28 inches. In the 24 hours *preceding* this time, there had fallen eight tenths of an inch of rain; in the 24 hours *following* it there fell none, nor was the wind, which blew from S.W., at all strong; indeed it was calm all the middle part of the day, with sunshine and cirrus above: evaporation was very perceptible, and the night, up to 10 p.m. starlight. The barometer, at 8 p. m. the 25th, was at 28.40 in. In the early morning of the 27th, not having yet reached 29 in., it turned to fall again, with the wind at S. and S.W., after S.E.: we had again some heavy rain with hail about noon, and by midnight the quicksilver reached 28.07, or .06 in., where it stood, or rather made minute oscillations, *during the* 12 *hours following*, a thing I should scarcely have thought possible in our climate.

It was stormy, with much rain, and cloudy during most of this interval; but at noon on the 29th, and from the above-mentioned very low point, the decisive rise began; which proceeding in a bold uninterrupted curve into the afternoon of the 31st, the quicksilver once more touched upon 30 inches, with the winds northerly, and moderate; and the year went out with fine weather.

Such were the principal circumstances which met my notice in a depression of the barometer, to which I find no parallel, for London, in the whole Meteorological Annals of the Society. Let us, however, now advert to a case or two which seem to have approached to it. BARKER, of Lyndon, gives the following monthly minima, viz. 1782, April, 28.09 in.; 1783, February, 28.08; and March, 27.88 in. (Philosophical Transactions, Vol. LXXIII., p. 242, and LXXIV.,

p. 283.) Now, the two latter occurred during the dreadful earthquakes in Calabria, of which we have a record in the Transactions; and I believe the barometer was noted to be extremely low, about that time, in various distant parts of England; but a comparison of the Society's Register is here precluded by a chasm of several years continuance. I have no doubt, however, from the general appearance of the means in the Lyndon Register, that the barometer there stood commonly some tenths lower than that at Somerset house. With respect to my own, I found yesterday, when the quicksilver was but little above 30 in., that my portable barometer exceeded that at Somerset House, when placed by its side, by 0.05 in., which was likewise, as nearly as I could judge, the difference in excess from my clock barometer. The latter therefore agrees very nearly, in this part of the scale at least, with the barometer registered in the Transactions.

I annex to this paper a diagram, traced from the variation on the face of my clock, for the two latter months of 1821, the scale being three quarters to an inch, (Pl. XIII.) placing at bottom the amount of rain in each successive five days, and the winds, so far as may serve to show their succession. It will be seen that this great depression was preceded by abrupt changes, fluctuating for 30 days, chiefly between 29.5 and 30 inches, during a continuance of stormy weather; and that the depression itself was 14 or 15 days in progress, from the point of 30 inches, to that from which it finally rose in three days.

The rain for these two months is 10.10 inches, a quantity without precedent in the same space of time at London: that is to say, without one on record. BARKER gives, however,

for "April, 1782, 6.125 in.; and for "May, 5.722 in." at Lyndon; yet the South Lambeth table, given along with his own, exhibits but 6.24 in. for these two months; and states 6.88 in. for the seventh month (July), following, where he has but 2.70 in. I am almost at a loss for an apology to the Society, for having in my last paper anticipated, on the strength of a single analogy, a *dry* year for 1821, the fact being, that there has fallen at Tottenham, in the whole year, no less than 33.84 inches. It seems as if, with all our anxiety to pass the stream of uncertainty in this science, we must give over making the wooden bridges of conjecture, and wait till we can accumulate more solid materials.

Tottenham Green,
First Month 10*th*, 1822.

XIV. *On the anomalous magnetic action of hot iron between the white and blood-red heat. By* Peter Barlow, *Esq. of the Royal Military Academy. Communicated by Major* Thomas Colby, *of the Royal Engineers, F. R. S.*

Read January 24, 1822.

In consequence of certain theoretical results relative to the magnetic action of iron, obtained by Mr. Charles Bonnycastle, I was desirous of ascertaining the relative attraction which different species of iron and steel had for the magnet; and with this view I procured two bars of each of the following descriptions of metal, 24 inches in length, and 1 inch and a quarter square, which being placed successively in the direction of the dip, at a certain distance from the compass, the disturbance occasioned by each was carefully noted; first with one end upwards, and then with the other; and assuming the tangents of the angles as the measure of the disturbing power, I obtained the following specific results, viz.

	Mag. Pow.		Mag. Pow.
Malleable iron - -	100	Shear steel soft - -	66
Cast iron - - - -	48	——— hard - -	53
Blistered steel soft -	67	Cast steel soft - -	74
——— hard -	53	——— hard - -	49

As it was obvious from these experiments, that the softer the iron the greater was its power, and the contrary, I was desirous of determining how nearly these different kinds of metal would approximate towards each other in their

magnetic action, when rendered perfectly soft by being heated in a furnace. With this view, bars of equal size of cast iron, malleable iron, shear steel, &c. were rendered white hot, and being placed in the direction of the dip, as before, their powers, as was anticipated, agreed nearly with each other; but still the cast iron, which was weakest while the metal was cold, exceeded a little in power all the others when hot, and the malleable iron which had the greatest power cold, had the least when hot; but the difference was not very great, and might probably arise from some accidental circumstance. While carrying on these experiments, it had been observed, both by Mr. BONNYCASTLE and myself, that between the white heat of the metal, when all magnetic action was lost, and the blood-red heat, at which it was the strongest, there was an intermediate state in which the iron attracted the needle the contrary way to what it did when it was cold, viz. if the bar and compass were so situated that the *north* end of the needle was drawn towards it when cold, the *south* end was attracted during the interval above alluded to, or while the iron was passing through the shades of colour denoted by the workman the bright red and red heat.

As this anomalous action had never before been noticed, I was desirous of examining it a little more particularly, and with the assistance of Mr. BONNYCASTLE, the following series of experiments were performed, wholly directed to this inquiry. Before entering upon the detail, however, it may not be amiss to notice those results which have hitherto been obtained relative to the magnetic action of heated iron; and to show how the contradictory statements, that we find on the subject, may be reconciled with each other. For example: we

find it stated in NEWTON's optics, that red hot iron has no magnetic property; while Father KIRCHER asserts, that the magnet will attract red hot iron as well as cold, ("de Magnete" lib. 1). Again, in Vol. XVIII. No. 214. Phil. Trans. it is stated, that hot iron not only has an attraction for the magnet, but that its power is increased by the heat; and these assertions have been repeated by many other authors, each supposing that his results were at variance with the other.

M. CAVALLO seems to have been the first writer, who was fully aware that these contradictory statements arose from the observations being made with the iron at different degrees of heat. He found, that although iron at the red heat had a greater power over the magnet than when cold, yet, at the white heat, it had a less; but he says he is still unable to decide, whether all the magnetic power is intirely lost at the white heat. (CAVALLO on Magnetism, p. 312.) More recent experiments on this subject are also recorded in Vol. IX. Part I. of the Transactions of the Royal Society of Edinburgh, by WILLIAM SCORESBY, Esq. But even here it does not appear that this Gentleman was aware of the total loss of power at a certain temperature; for he observes, (after showing that iron red hot has a greater power than when cold) "The contrary to this has, I think, been generally asserted:" from which it would seem, that he had not heated his iron to a sufficient degree to detect the non-action at the white heat.

Notwithstanding therefore all the experiments that have been made, it is pretty evident from the above remarks, that considerable uncertainty still hangs over the results; arising, without doubt, from the want of proper conveniences for heating bars of sufficient size, and to a proper degree of intensity,

whereby one author has noticed one fact, and another a different one, without being aware how much depended upon a very slight change in the temperature of the iron.

On these grounds therefore it is presumed, that the following experiments would be entitled to some notice, as they serve to reconcile all these apparently contradictory statements; but the principal reason which has induced me to lay them before the Royal Society is, the anomalous action which they have been the means of discovering, while the iron passes through the shades of bright red and red, already alluded to in the preceding part of this paper, and which, to the best of my knowledge, has never been noticed by any author.

Experiments on the anomalous attraction of heated iron which takes place while the metal retains the bright red and red heat.

I have already observed, that this anomalous action was noticed while we were pursuing other experiments, and that those which follow, were wholly directed to an examination of these irregularities.

In our first experiment, the compass was placed nearly west of the bar, rather below its upper extremity, and distant from it about $6\frac{1}{2}$ inches. At the white heat the attraction of the iron was lost; and at the blood red heat we had 70° of deviation in the needle; but that intermediate action we were searching after did not appear; at least it was by no means so obvious as we had noticed it in our preceding experiments.

The position of the bar and compass was not however quite the same as before; we therefore raised the support of the bar

about 4 inches, by which means its upper extremity was the same height above the compass, and on repeating the experiment with the bar thus placed, we obtained an obvious deviation of the south end of the needle towards the iron of $4\frac{1}{2}°$, which remained fixed about two minutes.

Having gained this by raising the bar 4 inches, we now raised it 6 inches, and on applying it in this place, we obtained a deviation of $10\frac{1}{2}°$, which remained fixed about the same time as before; when the needle suddenly yielded to the natural magnetic power of the iron, and obtained almost instantaneously a deviation of 81° the opposite way.

It was thus rendered obvious, that the quantity of negative attraction at the red heat, depended upon the height or depth of the centre of the bar from the compass; and as the natural effect of the cold iron was changed by placing the compass below the centre of the bar, the question which naturally suggested itself was, will the negative attraction also change? To decide this point, we lowered our compass to within 6 inches of the bottom of the bar; in which position the cold iron necessarily attracted the south end of the needle, and produced a deviation of 21°. Upon heating the bar, we found, as usual, all its power upon the needle cease at the white heat; but as this subsided into the bright red, the negative attraction began to manifest itself, and it soon amounted to $10\frac{1}{2}°$; the north end of the needle being attracted towards the iron. Here it remained stationary a short time, and then gradually returned, first due north, and ultimately to 70° 30′ on the opposite side.

Having made these preliminary experiments, I was anxious to undertake a regular series, hoping by this means to be

able to reduce this species of action to some fixed principle; for it will have been observed, from what is stated above, that the negative attraction appeared to increase from each extremity of the bar towards its middle; whereas the positive or natural action of the iron decreases in the like cases, and (passing through zero in the plane of no attraction) has its quality of attraction different when placed towards the upper or lower extremity of the bar.

The negative attraction has also the same change of character in the upper and lower extremity of the bar; but as it increases towards the middle, it appeared to pass through a maximum to arrive at that change, which seemed wholly inexplicable; and I must acknowledge that, after all the experiments I have made, it still remains so. It is at all events certain, that the least change of position of the compass when near the centre of the bar, changes altogether the quantity and quality of this negative action.

In the experiments detailed in the following table, I used four different bars, each 25 inches long, and $1\frac{1}{4}$ inch square; two of them of cast iron, denoted in the first column by C. B, No. 1; C. B, No. 2; and two of malleable iron, denoted by M. B, No. 1, and M. B, No. 2.

I had also two other bars, one of cast and one of malleable iron, of the same dimensions, which were not heated, but kept as standards for determining the quantity of cold attraction, as this could not safely be done by the bars used in the experiments after being so repeatedly heated.

The time employed in each experiment was about a quarter of an hour: the white heat commonly remained about 3 minutes, when the negative attraction commenced; this

lasted about two minutes more, when the usual attraction took place: this sometimes arrived at its maximum very rapidly, but at others it proceeded increasing very gradually; and commonly within the time stated above, the needle had been found perfectly stationary.

In the table, to avoid confusion, that attraction which took place according to the known laws of cold iron is marked *plus*, whichever end of the needle approached the iron, and the opposite attraction is marked *minus*. For example, when the compass is above the centre of the bar, the north end of the needle should be drawn towards the iron, but when the compass is below the centre, the south end should approach the iron; these therefore are both marked *plus*, and the contrary attraction at the red heat is marked *minus*.

TABLE,

Showing the effect of iron on the compass needle at different degrees of heat.

Height or depth of centre of bar from compass.	Distance of bar from compass	Position of compass.	Effect Cold.	Effect White Heat.	Effect Red Heat.	Effect Blood red Heat.	REMARKS.
Inch.	Inch.		° ′	° ′	° ′	° ′	
0.0	6.0	S. 80° W.	+ 0 0	0 0	— 17 0	0 0	South end drawn to the bar at red heat.
4.5 below.	6.0	ditto.	+30 0	ditto.	0 0	+45 0	
4.5 below.	6.0	ditto.	+18 0	ditto.	0 0	+49 0	
ditto.	6.0	ditto.	+29 30	ditto.	— 12 0	+44 0	
13 below.	6.0	ditto.	Not obs.	ditto.	0 0	+52 0	This bar being left standing, it attracted
4.5 below.	6.0	N. 80 W.	ditto.	ditto.	— 12 30	+70 0	the same three days after.
4.5 above.	6.0	S. 80 W.	ditto.	ditto.	— 12 30	+30 0	
ditto.	6.0	ditto.	ditto.	ditto.	0 0	+25 0	The needle suspected to touch the box.
ditto.	6.0	ditto.	ditto.	ditto.	— 19 0	+30 0	
1.0 above.	6.0	ditto.	ditto.	ditto.	— 15 0	+ 4 0	
2.5 below.	8.5	N. 80 W.	+29 30	ditto.	0 0	+37 30	Observed at the same time with two com-
ditto.	8.5	N. 80 E.	+30 0	ditto.	0 0	+41 0	passes.
2.5 below.	8.5	N. 80 W.	+16 0	ditto.	0 0	+42 30	Ditto.
ditto.	8.5	N. 80 E.	+15 30	ditto.	0 0	+47 30	
3.0 below.	8.5	N. 80 W.	+28 30	ditto.	— 1 0	+39 30	Ditto.
ditto.	8.5	N. 80 E.	+29 30	ditto.	— 1 30	+42 0	
9.0 below.	8.5	N. 80 W.	+15 45	ditto.	— 1 30	+45 0	Ditto.
ditto.	8.5	N. 80 E.	+16 0	ditto.	— 1 30	49 0	
5.0 below.	8.5	N. 80 W.	+25 0	ditto.	— 3 0	+32 30	Ditto.
ditto.	8.5	N. 80 E.	+26 0	ditto.	— 3 30	+33 0	
5.0 below.	8.5	N. 80 W.	+11 30	ditto.	— 3 30	+36 30	Ditto.
ditto.	8.5	N. 80 E.	+13 0	ditto.	Not obs.	+36 30	
3.0 below.	6.0	S. 80 E.	+ 8 0	ditto.	— 21 30	Not obs.	Ditto.
ditto.	6.0	N. 45 W.	Not obs.	ditto.	— 25 30	+25 30	
).0	6.0	ditto.	0 0	ditto.	— 40 0	0 0	North end drawn to the bar at red heat.
1.0 above.	5.3	N. 60 W.	+ 2 0	ditto.	— 4 30	+ 5 30	Both attractions very gradual.
ditto.	5.3	ditto.	Not obs.	ditto.	— 12 30	+ 5 30	Passed suddenly to 12½°, but returned immediately.
).0 above.	6.0	N. 85 E.	+47 30	ditto.	— 2 30	+60 0	Attractions gradual.
ditto.	6.0	ditto.	+47 30	ditto.	— 2 30	+60 0	Ditto.
1.0 below.	5.5	N. 45 W.	Not obs.	ditto.	— 55 0	+ 5 45	Negative attraction rather sudden.
4.5 above.	7.0	N. 75 E.	ditto.	ditto.	— 2 30	+33 30	Motion of needle very slow.
1.7 below.	5.5	N. 45 W.	ditto.	ditto.	+100 0	+13 30	100° very sudden, returned immediately.
1.7 above.	5.5	ditto.	ditto.	ditto.	— 26 0	+13 30	Both attractions gradual.
1.7 above.	5.5	ditto.	ditto.	ditto.	+ 30 0	+13 30	The same as No. 32; both anomalous.
4.5 above.	6.0	N. 55 E.	ditto.	ditto.	— 5 30	+35 30	Attractions very gradual.
ditto.	6.0	ditto.	ditto.	ditto.	— 0 0	+35 30	
).0	4.7	West.	+ 3 30	ditto.	— 50 0	+ 8 0	Motion regular, but quick.
).0	4.7	North.	0 0	ditto.	0 0	0 0	No motion in the needle.

It should be observed, that all the above experiments were made with the bars inclined in the direction of the dipping needle, or nearly in that direction, and it will be seen that the negative attraction was the greatest where the natural attraction was the least; that is, opposite the middle of the bar, or in the plane of no attraction. I was led, therefore, to make a few experiments with the bar inclined at right angles to its former position, but the results were by no means so strongly marked as in the preceding experiments: we always found a certain quantity of negative attraction, but it was very inconsiderable, never amounting to more than $2\frac{1}{2}°$.

I also made one experiment with an iron 24lb. ball, but the heat was too intense to make any very accurate observation. The numbers obtained were

Cold attraction $+13° 30'$.	White heat $0° 0'$.
Red heat $-3° 30'$.	Blood-red heat $+19° 30'$.

It may be proper also to state, that, being doubtful how far the heat itself, independent of the iron, might be the cause of the anomalous action above described, I procured two copper bolts of rather larger dimensions than my iron bars, and had them heated to the greatest degree that metal would bear; but on applying them to the compass, no motion whatever could be discovered in the needle.

The only probable explanation which I can offer by way of accounting for these anomalies, is, that the iron cooling faster towards its extremities than towards its centre, a part of the bar will become magnetic before the other part, and thereby cause a different species of attraction; but I must acknowledge, that this will not satisfactorily explain all the observed

phenomena. The results, however, are stated precisely as they were noted during the experiments, and others more competent than myself will probably be able to deduce the theory of them.

XV. *Observations for ascertaining the length of the Pendulum at Madras in the East Indies, latitude* 13° 4′ 9″,1 *N. with the conclusions drawn from the same. By* JOHN GOLDINGHAM, *Esq. F. R. S.*

Read January 31, 1822.

THE object of the enquiry in this paper has been considered at all times one of interest and importance, and is particularly so at present, when investigations have been completed in Europe, by order of some of the governments there; such as, with reference to their accuracy, had never before been made in any quarter of the globe, so far as comes within my recollection. I had seen the details of Captain KATER's experiments in the Philosophical Transactions, and he also did me the favour to send me out a copy of his Paper. The simplicity and accuracy of the apparatus induced me to write to that Gentleman, requesting he would have the goodness to order a similar one to be sent to me. This request he not only most readily complied with, but made the experiments requisite for enabling me to draw the conclusions; and thence to form the comparison with the results obtained in Europe. The apparatus arrived in March, and I immediately set about fixing it; which, notwithstanding the little solid assistance to be obtained in an operation of this nature from workmen in this country, I was enabled to effect in a most satisfactory manner; and I am led to hope, these observations will not be deemed unworthy the attention of the learned in Europe.

The clock used in these experiments has a gridiron pendulum, the motion being given by a spring; the maker's name is HASWELL, and the works are of the best description: it was fixed to the north wall of the Observatory, which is of solid masonry two feet in thickness: the rate was ascertained by comparisons with the transit clock each day, at the commencement and conclusion of the experiments: the transit of the noon before the comparison, and that after, were used in finding this rate; so that four results were obtained from the two comparisons: the transit clock, which is an excellent time keeper, was regulated by transits of the sun and stars; the weather fortunately having been clear, both at noon and at night, during the time the experiments were making.

The first operation performed was that of making the weight of the clock pendulum black, and fixing the disc on the centre. This having been done, five blocks of seasoned teak-wood, each 4 inches in diameter and 7 in length, were prepared; the place above the clock for the frame, which was to support the pendulum, was then marked. This I did with great care and precaution: intersecting lines were drawn upon the wall to show the exact position of the centres of the blocks and of the screws for fastening the frame; holes 4 inches in diameter and $10\frac{1}{2}$ in depth, (it being necessary to let the outer part of the blocks $3\frac{1}{2}$ inches within the surface of the wall, to bring the pendulum sufficiently near the clock case) were then made in the wall, and the blocks, coated with tar to preserve them from the white ants, were let in and firmly secured. The outer surfaces of the whole, which had previously been made smooth and level, being in

one plane. The frame, its two parts being firmly screwed together, was then placed, levelled by means of a spirit level, and fixed to the blocks in the firmest manner: the frame enclosing the agates was next put up, levelled, and screwed in its place, the Y's elevated, and the pendulum hung; the knife edges were then lowered upon the agates; when I had the satisfaction to find, from the precautions which I had taken, that the pendulum was most correctly in its place. In this distant part of the globe, there is an anxiety in handling and fixing any new apparatus which is not felt in England, where the maker of it is ready to give assistance, as well as to repair any damage that may chance to have been done: here, little or no assistance can be obtained; and if the use of any part of the apparatus should be mistaken, and the part forced into a wrong place, the injury may be fatal to the experiments, as it cannot be repaired here; it therefore affords no small gratification when an instrument is firmly secured, uninjured, in its proper position.

The pendulum is precisely the same, in all its parts, as that used by Captain KATER at the different stations of the Trigonometrical Survey of England, and which he has fully described in the Philosophical Transactions for 1819. Any farther description therefore of its construction, will here be unnecessary.

The next operation was to fix the arc for measuring the vibrations. The clock-case was of handsome mahogany enriched with projecting mouldings, with the door in front of plate glass. The mouldings kept the pendulum at too great a distance from the part of the case where the arc could otherwise have been fastened, and it became necessary to

have a support in front of the case. I therefore had a solid stand of teak wood made, similar to that for supporting the telescope, the inner part cut out to the form of the mouldings of the clock-case, so that it fitted perfectly close to it; in this position it was screwed to the floor; the ends for the supports of the arc were then let in, and secured to the top of the stand, and the arc fixed in its proper place, with reference to the extreme point of the pendulum. The floor outside of this apparatus was then separated from the part of the floor which supported it, to prevent any shake by persons moving about within the building.

The small telescope containing the diaphragm was now fixed upon its stand, and screwed to the floor at the proper distance from the pendulum: this was about $9\frac{1}{2}$ feet. The telescope, and every other part of the apparatus, have been so fully described by Captain KATER in the paper published in the Philosophical Transactions, that I feel it unnecessary to be more particular here.*

While making the holes in the wall above the clock for the insertion of the blocks for supporting the frame, a great deal of dust would necessarily fall upon the clock-case; every part of the case where dust could penetrate was therefore filled up with wax, and several folds of cloth were afterwards secured over the whole of the case, so that it was hardly possible that any dust could penetrate to the works of the clock. After the frame was fixed, the cloth and wax were removed, and fresh oil applied to the works.

* The drawing [Pl. XIV] shows the inside of a part of the Observatory, the pendulum up, and the adjustment of the diaphragm making, preparatory to commencing the observations.

The clock was then set in motion. This was on the 22d of March, and the observations commenced two days afterwards.

The following is the mode pursued in making the observations.

The pendulum was lifted up from the Y's by myself and an assistant, and the knife edges wiped with a cloth saturated with oil. The pendulum was then replaced, and the Y's lowered, so that the knife edges rested upon the agates. The telescope was then adjusted (care being taken that the O on the arc of vibration coincided with the point of the slip), so that the edges of the slip were exactly embraced by the edges of the diaphragm. The height of the barometer, of the thermometer fixed near the middle of the pendulum, and that of the hygrometer, were taken and registered. The point of the slip at the end of the pendulum was then brought and kept by the hand to about 1°,3 upon the arc; and an instant before the pendulum of the clock was at its highest point on the same side, the hand was withdrawn, and the pendulum thereby allowed to vibrate freely. I stationed the head Bramin assistant* to take down the time, and the youngest Bramin assistant to count the clock, which he does with the greatest correctness. Having placed myself at the telescope, I found there was a sensible portion of time, more or less, as the arc of vibration was greater or smaller, between the disappearance of the disc behind the slip, and its reappearance; I therefore noticed the seconds, and parts of a second, when the disc disappeared, and also the instant when it

* The name of the head assistant is Senavassachary, and that of the other Teroovencatachary.

again appeared, both which the Bramin put down; the mean of these I took as the true time of the coincidence, and registered it accordingly. These times I found could be accurately noted; and it is probable the mean of the two observations is generally correct to less than half a second. In this manner the times of the coincidences were observed. The thermometer often varying a good deal in a short time, I thought it right to take its height three times, at the third observation of each set, as well as the first and fifth. The barometer was observed at the end of each set, as well as at the beginning; and also the hygrometer, as mentioned above, at the beginning of the observations, and likewise at the end of those of each day; being desirous of seeing how much the atmosphere had changed in dryness, as well as in heat and weight; not that this was material, but it is satisfactory to know what change there actually was in the atmosphere during the time the observations were making. I now proceed to detail the observations.

Every observation taken is here given. As although in this as in similar cases, one feels better pleased with some observations than with others, yet I do not recollect more than two of these observations, which I felt dissatisfied with at the time, and that not in a sufficient degree to induce me to think of rejecting them.

The results, I trust, will prove how unnecessary it would have been to have rejected any of the observations.

OBSERVATIONS.

FIRST SERIES.

	h.	m.	Inch.
March 24th A. M.	6	24	30,085
	7	12	30,101

Rate of the Clock —0″,39. Hygrometer 11°,5 dry. Mean - 30,093

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
°	h. m. s.	°	°	″			+	+	
81	6 23 44	1,15							
			1, 10	721,25	719,25	86160,42	1,983	4,687	86167,090
	35 45,25	1,05							
			1, 01	722,75	720,75	86160,91	1,671	4,750	86167,331
81,3	47 48	0,97							
			0,935	724	722	86161,33	1,432	4,793	86167,555
	59 52	0,88							
			0,835	723, 8	721, 8	86161,26	1,102	4,814	86167,216
81,4	7 11 55,8	0,79							
81,23	Mean								86167,298
						Rate of the Clock			— 0,390
									86166,908

Rate of the Clock —0″,39

Barometer 30,105; 30,123

Mean - 30,114

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
81,5	7 24 50,5	1,13							
			1,065	722, 4	720, 4	86160,80	1,858	4,809	86167,467
	36 52,9	1,00							
			0, 95	725, 7	723, 7	86161,88	1,479	4,915	86168,274
82	48 58,6	0,90							
			0, 87	723,65	721,65	86161,21	1,240	5,152	86167,602
	8 1 2,25	0,84							
			0,795	723,75	721,75	86161,24	1,036	5,300	86167,576
82,7	13 6,0	0,75							
81,73	Mean								86167,729
						Rate of the Clock			—,390
									86167,339

Barometer $\begin{cases} 30{,}137 \\ 30{,}134 \end{cases}$

Mean - 30,135

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
°	h. m. s.	°	°	″			+	+	
82,9	8 29 26,5	1,15							
			1,10	720, 6	718, 6	86160,20	1,983	5,512	86167,692
	41 27,1	1,05							
			1,00	725, 0	723, 0	86161,66	1,638	5,592	86168,888
83,4	53 32,1	0,95							
			0,91	722,65	720,65	86160,88	1,357	5,765	86167,995
	9 5 34,75	0,87							
			0,83	722,65	720,65	86160,88	1,129	5,956	86167,996
84,3	17 37,4	0,79							
83,53	Mean						Rate of the Clock		86168,135 — 0,390
									86167,745

Barometer $\begin{cases} 30{,}129 \\ 30{,}140 \end{cases}$

Mean - 30,135

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84,5	9 33 39,25	1,11							
			1, 07	719,35	717,35	86159,78	1,876	6,188	86167,844
	45 38,6	1,03							
			0, 99	718,15	716,15	86159,38	1,605	6,290	86167,275
85	57 36,75	0,95							
			0,915	718,75	716,75	86159,58	1,372	6,345	86167,297
	10 9 35,5	0,88							
			0, 84	719, 5	717, 5	86159,83	1,156	6,345	86167,331
85	21 35,0	0,80							
84,83	Mean						Rate of the Clock		86167,437 — ,390
									86167,047

Hygrometer 13, dry

Barometer { 30,140 / 30,143

Mean - 30,141

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
°	h. m. s.	°	°	″			+	+	
84,9	10 30 16,6	1,20							
			1,165	713,65	711,65	86157,86	2,224	6,315	86166,399
	42 10,25	1,13							
			1,085	715,25	713,25	86158,01	1,929	6,332	86166,654
85	54 5,5	1,04							
			1,015	717,0	715, 0	86159,00	1,688	6,345	86167,033
	11 6 2,5	0,99							
			0,945	715,65	713,65	86158,64	1,463	6,345	86166,348
85	17 58,15	0,90							
84,97	Mean						Rate of the Clock		86166,608 — ,390
									86166,218

March 25, A. M.

Clock losing 1″,49
Hygrometer 12°. dry

Barometer { 30,143 / 30,147

Mean - 30,145

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
°	h. m. s.	°	°	″			+	+	
80,3	6 14 0,25	1,24							
			1,205	717,5	715, 5	86159,16	2,379	4,433	86165,972
	25 57,75	1,17							
			1,130	720,25	718,25	86160,08	2,092	4,581	86166,753
81,0	37 58	1,09							
			1,055	720,5	718, 5	86160,03	1,826	4,674	86166,530
	49 58,5	1,02							
			0,980	722,6	720, 6	86160,86	1,574	4,716	86167,150
81,2	7 2 1,1	0,94							
80,83	Mean						Rate of the Clock		86166,601 — 1,490
									86165,111

End of the Experiments. Barometer { 30,147 / 30,161 }

Hygrometer $12^{\circ}\frac{1}{8}$ dry. Mean - 30,154

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
81,2	7 10 19,5	1,28							
			1, 23	719,1	717,1	86159,70	2,479	4,780	86166,959
	22 18,6	1,18							
			1,125	722	720	86160,66	2,073	4,864	86167,597
81,6	34 20,6	1,07							
			1, 02	723	721	86160,99	1,705	4,949	86167,644
	46 23,6	0,97							
			0, 91	724,5	722,5	86161,49	1,357	5,245	86168,092
82	58 28,1	0,85							
81,6	Mean						Rate of the Clock		86167,573 — 1,490
									86166,083

March 26, P. M.

Barometer { 30,132 / 30,133 }

Clock losing 1″,91
Hygrometer 15°,6 dry

Mean - 30,133

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84	4 58 37,1	1,20							
			1,15	716, 5	714, 5	86158,83	2,167	5,913	86166,910
	10 33,6	1,10							
			1,07	717,85	715,85	86159,28	1,876	5,888	86167,044
83,9	22 31,45	1,04							
			1,01	719,05	717,05	86159,68	1,671	5,837	86167,188
	34 30,5	0,98							
			0,94	720,	718, 0	86160,00	1,448	5,753	86167,201
83,5	46 30,5	0,90							
83,8	Mean						Rate of the Clock		86167,086 — 1,910
									86165,176

March 26, A. M.

Clock losing 2″,08
Hygrometer 14°, dry.

Barometer { 30,149 / 30,150
Mean - 30,149

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
79,3	5 58 58,1	1,19							
			1, 14	723,27	721,27	86161,08	2,129	3,955	86167,164
	11 1,37	1,09							
			1,045	725,38	723,38	86161,70	1,789	3,997	86167,466
79,5	23 6,75	1,00							
			0,965	725, 5	723, 5	86161,81	1,526	4,048	86167,384
	35 12,25	0,93							
			0, 90	727, 2	725, 2	86162,37	1,327	4,116	86167,813
79,8	47 19,45	0,87							
79,53	Mean						Rate of the Clock		86167,457 — 2,080
									86165,377

Hygrometer 13°,6 dry.

Barometer { 30,150 / 30,173
Mean - 30,162

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
79,8	6 56 30,62	1,29							
			1, 24	721,88	719,88	86161,63	2,519	4,264	86168,413
	7 8 32,5	1,19							
			1, 14	724, 0	722, 0	86161,32	2,129	4,496	86167,945
80,9	20 36,5	1,09							
			1,045	724,17	722,17	86161,38	1,789	4,653	86167,822
	32 40,67	1,00							
			0,965	725,33	723,33	86161,76	1,526	4,738	86168,024
81,3	44 46,	0,93							
80,67	Mean						Rate of the Clock		86168,031 — 2, 08
									86165,971

March 27, P. M.

Clock losing 2″,06
Hygrometer 20° dry.

Barometer 30,116 / 30,131
Mean - 30,123

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84, 7	4 13 44,25	1,24							
			1, 20	715,87	713,87	86158,62	2,359	6,176	86167,155
	25 40,12	1,16							
			1, 12	716,51	714,51	86158,83	2,055	6,091	86166,976
84, 3	37 36,63	1,08							
			1,035	717,87	715,87	86159,29	1,755	6,028	86167,070
	49 34, 5	0,99							
			0,955	717,87	715,87	86159,28	1,494	5,985	86166,759
84, 1	5 1 32,37	0,92							
84,37	Mean						Mean of the Clock		86166,990
									— 2,060
									86164,930

Hygrometer 19°,3 dry.

Barometer 30,131 / 30,087
Mean - 30,109

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84,1	5 10 14,75	1,28							
			1,235	716,50	714,50	86158,83	2,499	5,943	86167,272
	22 11,25	1,19							
			1, 14	718,50	716,50	86159,50	2,129	5,901	86167,530
83,9	34 9,75	1,09							
			1,045	718,87	716,87	86159,62	1,789	5,837	86167,246
	46 8,62	1,00							
			0, 96	720,63	718,63	86160,21	1, 51	5,753	86167,473
83,5	58 9,25	0,92							
83,83	Mean						Rate of the Clock		86167,380
									— 2,060
									86165,320

March 29, P. M.

Clock losing 1″,46
Hygrometer 17°,7 dry.

Barometer { 30,084 / 30,113

Mean - 30,098

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84,5	2 59 8	1,23							
			1,19	719,12	717,12	86159,71	2,320	6,112	86168,142
	3 11 7,12	1,15							
			1,11	720,88	718,88	86160,29	2,019	6,070	86168,389
84,3	23 8,0	1,07							
			1,02	723, 0	721, 0	86160,99	1,705	6,028	86168,723
	35 11,0	0,97							
			0,93	723,63	721,63	86161,20	1,417	5,985	86168,602
84,1	47 14,63	0,89							
84,3	Mean						Rate of the Clock		86168,464 — 1,460
									86167,004

Hygrometer 17°,7 dry.

Barometer { 30,113 / 30,087

Mean - 30,100

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84,1	3 56 44,75	1,26							
			1,215	721, 0	719, 0	86160,33	2,418	5,956	86168,704
	48 45,75	1,17							
			1,115	721,75	719,75	86160,58	2,037	5,930	86168,447
84	20 47,50	1,06							
			1,015	723,62	721,62	86161,20	1,688	5,922	86168,810
	32 51,12	0,97							
			0, 93	723,13	721,13	86161,04	1,417	5,922	86168,379
84	44 54,25	0,89							
84,03	Mean						Rate of the Clock		86168,585 — 1, 46
									86167,121

March 30, P. M.

Clock losing 1″,96
Hygrometer 16°,2 dry.

Barometer 30,136 / 30,117
Mean - 30,126

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibration in 24 hours.
	h. m. s.	°	°	″			+	+	
84,4	2 57 1, 5	1,27							
			1,225	721, 0	719, 0	86160,33	2,459	6,083	86168,872
	3 9 2, 5	1,18							
			1,135	721,25	719,25	86160,42	2,111	6,057	86168,588
84,3	21 3,75	1,09							
			1,045	722, 5	720, 5	86160,83	1,789	6,049	86168,668
	33 6,25	1,00							
			0, 96	723,37	721,37	86161,12	1,510	6,049	86168,679
84,3	45 9,62	0,92							
84,33	Mean						Rate of the Clock		86168,702
									— 1,960
									86166,742

Hygrometer 16°,5 dry.

Barometer 30,117 / 30,115
Mean - 30,116

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibration in 24 hours.
	h. m. s.	°	°	″			+	+	
84,3	3 52 18,25	1,24							
			1, 19	720,62	718,62	86160,21	2,320	6,028	86168,558
	4 4 18,87	1,14							
			1,095	722,13	720,13	86160,71	1,965	5,985	86168,650
84,1	16 21	1,05							
			1,005	723,75	721,75	86161,24	1,655	6,008	86168,903
	28 24,75	1,96							
			0,925	723,63	721,63	86161,20	1,402	5,977	86 68,579
84	40 38,28	0,89							
84,13	Mean						Rate of the Clock		86168,675
									— 1,960
									86166,715

March 30, A. M.

Clock losing 2″,23
Hygrometer 13°,7 dry.

Barometer { 30,124 / 30,144
Mean - 30,134

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	° ′	°	′			+	+	
80,1	6 0 30	1,29							
			1,245	720, 5	718, 5	86160,17	2,539	4,327	86167,036
	12 30,5	1,20							
			1, 16	721,25	719,25	86160,41	2,205	4,433	86167,048
80,6	24 31,75	1,12							
			1, 08	721,87	719,87	86160,62	1,911	4,518	86167,049
	36 33,62	1,04							
			1, 0	723,50	721,50	86161,16	1,639	4,577	86167,376
80,9	48 37,12	0,96							
80,53	Mean							Rate of the Clock	86167,127 — 2, 23 86164,897

Hygrometer 13°,3 dry.

Barometer { 30,144 / 30,162
Mean - 30,153

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
81	6 59 7,12	1,19							
			1,145	722,75	720,75	86160,91	2,148	4,695	86167,753
	7 11 9,87	1,10							
			1, 05	723,63	721,63	86161,20	1,806	4,780	86167,786
81,4	23 13, 5	1, 0							
			0,965	724, 0	722, 0	86161,33	1,526	4,843	86167,699
	35 17, 5	0,93							
			0,905	724, 5	722, 5	86161,49	1,341	4,886	86167,717
81,6	47 22, 0	0,88							
81,33	Mean							Rate of the Clock	86167,739 — 2,230 86165,509

March 31, P. M.

Clock losing 2″,24
Hygrometer 16°,2 dry.

Barometer { 30,07 / 30,066
Mean - 30,068

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
85,1	3 8 27,5	1,24							
			1, 19	718,25	716,25	86159,42	2,320	6,366	86168,106
	20 25,75	1,14							
			1,095	719,37	717,37	86159,78	1,965	6,324	86168,069
84,9	32 25,12	1,05							
			1,005	720,38	718,38	86160,13	1,656	6,273	86168,059
	44 25, 5	0,96							
			0,940	719,75	717,75	86159,92	1,448	6,210	86167,578
84,6	56 25,25	0,88							
84,87	Mean						Rate of the Clock		86167,953
									— 2,240
									86165,713

Hygrometer 16°,3 dry.

Barometer { 30,066 / 30,061
Mean - 30,064

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84,6	4 6 55, 5	1,37							
			1,32	716,62	714,62	86158,87	2,855	6,155	86167,880
	18 52,12	1,27							
			1,22	717,75	715,75	86159,24	2,439	6,112	86167, 79
84,4	30 49,87	1,17							
			1,12	720,00	718,00	86160,00	2,055	6,074	86168,129
	42 49,87	1,07							
			1,02	719,75	717,75	86159,67	1,705	6,049	86167,424
84,26	54 49,62	0,97							
84,42	Mean						Rate of the Clock		86167,806
									— 2,240
									86165,566

March 31, P. M.

Clock losing 2″10
Hygrometer 14°,6 dry.

Barometer 30,098 / 30,114
Mean - 30,106

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
81,3	5 59 27, 5	1,21							
			1, 17	721, 5	719, 5	86160,50	2,243	4,763	86167,506
	6 11 28, 5	1,13							
			1,095	723,12	721,12	86161,03	1,964	4,733	86167,727
81,15	23 31,62	1,06							
			1,025	723,88	721,88	86161,29	1,721	4,725	86167,736
	35 35, 5	0,99							
			0,955	724,87	722,87	86161,61	1,494	4,750	86167,854
81,25	47 40,37	0,92							
81,23	Mean								86167,706
						Rate of the Clock			— 2,100
									86165,606

Hygrometer 13°,5 dry.

Barometer 30,114 / 30,130
Mean - 30,122

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
81,35	6 59 59, 5	1,20							
			1, 16	722,75	720,75	86160,91	2,205	4,822	86167,937
	12 2,25	1,12							
			1, 08	724,37	722,37	86161,45	1,911	4,864	86168,225
81,55	24 6,62	1,04							
			0,995	725,75	723,75	86161,90	1,622	4,941	86168,463
	36 12,37	0,95							
			0, 91	726,63	724,63	86162,19	1,357	5,021	86168,568
81,95	48 19,00	0,87							
81,62	Mean								86168,298
						Rate of the Clock			— 2,100
									86166,198

April 1, P. M.

Clock losing 1″85
Hygrometer 16°8 dry.

Barometer { 30,100 / 30 087
Mean - 30,094

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
85,3	2 23 30,62	1, 18							
			1,137	717,52	715,52	86159,17	2,118	6,473	86167,761
	35 28,12	1,095							
			1,048	720,13	718,13	86160,04	1,799	6,473	86168,312
85,3	47 28,25	1,000							
			0, 96	720,75	718,75	86160,25	1,510	6,455	86168,215
	59 29	0, 92							
			0,883	721, 5	719, 5	86160,50	1,278	6,425	86168,203
85,15	3 11 30, 5	0,845							
85,25	Mean						Rate of the Clock		86168,123
									— 1,850
									86166,273

Hygrometer 16°,8 dry.

Barometer { 30,087 / 30 c83
Mean - 30,085

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
85,15	3 19 39,75	1,27							
			1,21	719,25	717,25	86159,75	2,399	6,404	86168,553
	31 39	1,15							
			1,10	719,62	717,62	86159,87	1,983	6,396	86168,249
85,10	43 38,62	1,05							
			1,01	720,63	718,63	86160,21	1,671	6,383	86168,264
	55 39,25	0,97							
			0,93	722,00	720,00	86160,66	1,417	6,375	86168,452
85,06	4 7 41,25	0,89							
85,10	Mean						Rate of the Clock		86168,379
									— 1,850
									86166,529

April 1, P. M.

Clock losing 1″,86
Hygrometer 48° dry.

Barometer { 30,126 / 30,134
Mean - 30,130

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Contraction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
81,95	6 16 49,75	1, 19							
			1,145	722,25	720,25	86160,83	2,148	5,114	86168,092
	28 52,0	1, 10							
			1,055	723,13	721,13	86161,04	1,824	5,233	86168,097
82,5	40 55,13	1, 01							
			0,978	723,37	721,37	86161,12	1,567	5,300	86167,987
	52 58,5	0,945							
			0,912	723,62	721,62	86161,20	1,363	5,321	86167,884
82,6	7 5 2,12	0, 88							
82,35	Mean						Rate of the Clock		86168,015
									— 1,860
									86166,155

Hygrometer 13°,8 dry.

Barometer { 30,134 / 30,172
Mean - 30,153

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Contraction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
82,6	7 13 12,62	1,195							
			1,148	722	720	86161,00	2,159	5,355	86168,514
	25 14,62	1, 10							
			1, 06	723,88	721,88	86161,28	1,841	5,410	86168,531
82,85	37 18,5	1, 02							
			0, 98	725,12	723,12	86161,69	1,574	5,444	86868,708
	49 23,62	0, 94							
			0, 89	725,63	723,63	86161,86	1,321	5,469	86168,650
82,95	8 1 29,25	0,855							
82,80	Mean						Rate of the Clock		86168,601
									— 1,860
									86166,741

April 2, P. M.

Clock losing 2″09
Hygrometer 16°,7 dry.

Barometer { 30,135 / 30,127

Mean - 30,131

Tem.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
85,75	2 52 1,5	1, 23							
			1, 18	719,62	717,62	86159,87	2,281	6,620	86168,771
	3 4 1,12	1, 13							
			1,085	719,88	717,88	86159,96	1,929	6,535	86168,424
85,35	16 1,0	1, 04							
			0,993	721, 0	719, 0	86160,33	1,616	6,455	86168,401
	28 2	0,945							
			0, 91	720,12	718,12	86160,04	1,357	6,383	86167,780
85,0	40 2,12	0,875							
85,37	Mean							Rate of the Clock	86168,344 — 2,090
									86166,254

Hygrometer 16°7 dry.

Barometer { 30,127 / 30,118

Mean - 30,123

Tem.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.		°	″			+	+	
85	3 46 27,87	1,195							
			1,153	719,88	717,88	86159,96	2,178	6,324	86168,462
	58 27,75	1,110							
			1,052	720,25	718,25	86160,08	1,813	6,282	86168,175
84,8	4 10 28,0	0,995							
			0,958	722,12	720,12	86160,70	1,504	6,260	86168,464
	22 30,12	0, 92							
			0,885	722,03	720,03	86160,68	1,283	6,260	86168,223
84,8	34 32,15	0, 85							
84,87	Mean							Rate of the Clock	86168,331 — 2,090
									86166,241

The correction for the arc of vibration was ascertained by multiplying the square of the mean arc by 1,6385. The correction for temperature was found as follows: the mean of the thermometer at the beginning and middle of the observations was taken, and that of the middle and end; which gave five heights, one for each observation; the mean of the first and second, of the second and third, and so on in succession was taken, which gave four mean heights; the difference between each of these and 70° was multiplied by 0,423, the part of a vibration due to each degree of the thermometer, as furnished by Captain KATER, and the required correction was obtained.

The rate of the clock was found as before mentioned. The following shows the daily rate of the transit clock, in the interval during which the observations were taken; and furnishes a satisfactory example of the good performance of this standard for finding the rate of the other clock.

Rate of the Transit Clock.

March 22	— 0,25	March 29	+ 0,09
23	+ 0,13	30	+ 0,05
24	— 0,05	31	— 0,03
25	+ 0,23	April 1	+ 0,02
26	+ 0,10	2	— 0,08
27	+ 0,15	3	+ 0,05
28	+ 0,20	4	— 0,04

Table of the Results of the foregoing Experiments.

Day. 1821.	Time of the Experiment.	Mean Height of the Thermometer.	Mean Height of the Barometer.	Mean Height of the Hygrometer.	Number of Vibrations in 24 hours, at the temperature of 70° of Farenheit.
		°	Inch.	dry.	
March 24	A. M.	81,23	30,093		86166,908
		81,73	30,114		86167,339
		83,53	30,135		86166,275
		84,83	30,135	12,25	86167,047
		84,97	30,141		86166,218
25	A. M.	80,83	30,145		86165,111
		81, 6	30,154	12,22	86166,083
26	P. M.	83, 8	30,133		86165,176
	A. M.	79,53	30,149	14, 4	86165,377
		80,67	30,162		86165,971
27	P. M.	84,37	30,123		86164,930
		83,83	30,109	19, 6	86165,320
29	P. M.	84, 3	30,098		86167,004
		84,03	30,100	17, 7	86167,121
30	P. M.	84,33	30,126		86166,742
		84,13	30,116		86166,715
	A. M.	80,53	30,134	14,92	86164,897
		81,33	30,153		86165,509
31	P. M.	84,87	30,068		86165,713
		84,42	30,064	15,15	86165,566
	A. M.	81,23	30,106		86165,606
		81,62	30,122		86166,198
April 1	P. M.	85,25	30,094		86166,273
		85,10	30,085		86166,529
	A. M.	82,35	30,130	15,65	86166,155
		82,80	30,153		86166,741
2	P. M.	85,37	30,131		86166,254
		84,87	30,123	16, 7	86166,241
Mean		83,48	30,121	15,38	86166,108

Second Series of Experiments for ascertaining the length of the Pendulum at Madras.

Thinking it possible that these Observations might be referred to by future observers in other parts of the world, and wishing to have as accurate results as I could obtain, I deter-

mined to take a second series; having made what I considered some improvement in detaching the clock and apparatus from the floor of the building. In this series, besides comparisons for the rate of the clock used in the experiments, with the transit clock at the time of making the experiments, transits of stars were taken with this clock for the purpose. The result of this series, however, seems to prove, that every necessary precaution had been used in the first, the difference of the two being only 0,06 of a vibration in 24 hours.*

The following are the Observations of the Second Series.

OBSERVATIONS.

SECOND SERIES.

April 18th A. M.

Rate of Clock + 0″,97 } Hygrometer 12°,6 dry. }

Barometer { 30,018 / 30,029

Mean - 30,025

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For temperature.	Vibrations in 24 hours.
	h. m. s	°	°	″			+	+	
82,7	18 14 21,75	1,275							
			1,122	711,12	709,12	86157,00	2,447	5,402	86164,849
	26 12,87	1, 17							
			1,125	712,63	710,63	86157,52	2,074	5,469	86165,063
83	38 5,50	1, 08							
			1,035	713, 0	711, 0	86157,64	1,755	5,550	86164,945
	49 58,50	0, 99							
			0,953	713,62	711,62	86157,86	1,488	5,643	86164,691
83,45	19 1 52,12	0,915							
83,05	Mean						Rate of the Clock		86164,887 + 0,970
									86165,857

* By rejecting the 4 in each series, which differs most from the mean, we obtain a mean 0,03 of a vibration less than that from which the conclusions have been drawn.

Barometer $\begin{cases} 30,029 \\ 30,044 \end{cases}$

Mean - 30,037

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	"			+	+	
83,45	19 11 27,12	1, 30							
			1, 25	708,63	706,63	86156,15	2,560	5,748	86164,478
	23 15,75	1, 20							
			1,155	711,38	709,38	86157,09	2,186	5,863	86165,539
84,0	35 7,13	1, 11							
			1,067	711,12	709,12	86157,00	1,865	5,985	86164,850
	46 58,25	1,025							
			0,990	712,12	710,12	86157,34	1,606	6,112	86165,058
84,6	58 50,37	0,955							
84,02	Mean							Rate of the Clock	86164,981
									+ ,970
									86165,951

Hygrometer 15° dry.

Barometer $\begin{cases} 30,044 \\ 30,045 \end{cases}$

Mean - 30,044

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	"			+	+	
84,6	20 13 1, 5	1, 39							
			1,337	709, 0	707, 0	86156,28	2,929	6,218	86165,424
	24 50, 5	1,285							
			1,233	708,75	706,75	86156,19	2,491	6,303	86164,984
85	36 39,25	1, 18							
			1,132	710, 0	708	86156,66	2,100	6,396	86165,156
	48 29,25	1,085							
			1,043	709,37	707,37	86156,40	1,782	6,502	86164,684
85,5	21 0 18,62	1, 00							
85,04	Mean							Rate of the Clock	86165,062
									+ ,970
									86166,032

April 19, P. M.

Barometer $\begin{cases} 29{,}983 \\ 29{,}961 \end{cases}$

Mean - 29,972

Rate of Clock 0″88
Hygrometer 16°5 dry.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87,5	2 21 17,62	1,275							
			1.223	707,50	705,50	86155,76	2,451	7,386	86165,597
	33 5,12	1,170							
			1,112	708,50	706,50	86156,01	2,026	7,356	86165,392
87,35	44 53,62	1,055							
			1,015	710,88	708,88	86157,06	1,688	7,318	86166,066
	56 44, 5	0,975							
			0,933	709,62	707,62	86156,49	1,426	7,276	86165,192
87,15	3 8 34,12	0,890							
									86165,562
87,33	Mean						Rate of the Clock		+ ,880
									86166,442

Barometer $\begin{cases} 29{,}961 \\ 29{,}952 \end{cases}$

Mean - 29,956

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87,1	3 17 0,12	1, 26							
			1, 20	708,50	706,50	86156,01	2,359	7,212	86165,461
	28 48,62	1, 14							
			1,093	707,88	705,88	86155,89	1,958	7,170	86165,018
86,9	40 36, 5	1,045							
			1,013	710,62	708,62	86156,83	1,681	7,136	86165,647
	52 27,12	0, 98							
			0,935	710,50	708,50	86156,79	1,432	7,115	86165,337
86,8	4 4 17,62	0, 89							
									86165,366
86,93	Mean						Rate of the Clock		+ ,880
									86166,246

Hygrometer 16°5 dry.

Barometer {29,952 / 29,948

Mean - 29,950

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Contraction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	"			+	+	
86,8	4 11 57,4	1,19							
			1, 14	709,22	707,22	86156,35	2,129	7,064	86165,543
	23 46,62	1,09							
			1,045	710, 0	708, 0	86156,62	1,789	6,679	86165,388
86,4	35 36,62	1,00							
			0,955	711,75	709,75	86157,22	1,491	6,908	86165,619
	47 28,37	0,91							
			0,865	712,25	710,25	86157,39	1,226	6,840	86165,456
86,1	59 20,62	0,82							
86,43	Mean						Rate of the Clock		86165,502
									+ ,880
									86166,382

April 19, A. M. 18h 20 Civil.

Rate of Clock + 0″70
Hygrometer 13°4 dry.

Barometer {30,046 / 30,041

Mean - 30,044

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Contraction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	"			+	+	
83	17 54 47,62	1, 29							
			1,243	710,25	708,25	86156,705	2,532	5,562	86164,799
	18 6 37,87	1,195							
			1,150	711,50	709, 5	86157,132	2,167	5,689	86164,988
83,6	18 29,37	1,105							
			1,067	711,50	709,50	86157,132	1,865	5,753	86164,750
	30 20,87	1,030							
			0,995	713,63	711,63	86157,843	1,622	5,753	86165,218
83,6	42 14, 5	0, 96							
83,4	Mean						Rate of the Clock		86164,939
									+0,700
									86165,639

Barometer $\begin{cases} 30{,}041 \\ 30{,}050 \end{cases}$

Mean - 30,045

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
83,6	18 49 21	1,305							
			1,257	709,75	707,75	86156,533	2,589	5,795	86164,917
	19 1 10,75	1, 21							
			1, 16	712,37	710,37	86157,429	2,205	5,880	86165,514
84	13 3,12	1, 11							
			1, 07	711,38	709,38	86157,091	1,876	5,951	86164,918
	24 54, 5	1, 03							
			1, 00	713, 5	711, 5	86157,813	1,638	6,002	86165,453
84,25	36 48	0, 97							
83,95	Mean						Rate of the Clock		86165,200 + ,700
									86165,900

Hygrometer 13°,6.

Barometer $\begin{cases} 30{,}050 \\ 30{,}036 \end{cases}$

Mean - 30,043

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84,25	19 44 11, 5	1, 25							
			1, 21	710,25	708,25	86156,705	2,399	6,104	86165,208
	56 1,75	1, 17							
			1,125	711,37	709,37	86157,088	2,074	6,252	86165,414
84,95	20 7 53,12	1, 08							
			1,038	711,50	709,50	86157,132	1,765	6,341	86165,238
	19 44,62	0,995							
			0,958	712,13	710,13	86157,347	1,504	6,370	86165,221
85,1	31 36,75	0,920							
84,77	Mean						Rate of the Clock		86165,270 + ,700
									86165,700

April 20, P. M.

Rate of Clock + 0″,46
Hygrometer 16° 7 dry.

Barometer { 30,008 / 29,982 }
Mean - 29,995

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87,5	2 11 25, 5	1,195							
			1,148	707, 0	705, 0	86155,586	2,159	7,360	86165,105
	23 12, 5	1, 10							
			1,055	708	706	86155,932	1,824	7,276	86165,032
87,1	35 0, 5	1, 01							
			0,975	709,25	707,25	86156,362	1,557	7,233	86165,152
	46 49,75	0, 94							
			0,905	708,75	706,75	86156,190	1,342	7,233	86164,765
87,1	58 58, 5	0, 87							
87,23	Mean								86165,013
							Rate of the Clock		+ ,460
									86165,473

Barometer 29,982 / 29,977
Mean - 29,979

	h. m. s.	°	°	″			+	+	
87,0	3 5 32,62	1,205							
			1,158	707, 0	705	86155,586	2,197	7,191	86164,974
	17 19,62	1, 11							
			1,065	708,63	706,63	86156,149	1,858	7,191	86165,198
87,0	29 8,25	1, 02							
			0,985	708,75	706,75	86156,190	1,590	7,191	86164,971
	40 57, 0	0, 95							
			0, 92	710,62	708,62	86156,832	1,387	7,191	86165,410
87	52 47,62	0, 89							
87,0	Mean								86165,138
							Rate of the Clock		+ ,460
									86165,598

Hygrometer 17°,5 dry.

Barometer { 30,030 / 30,032

Mean - 30,031

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
86, 9	4 2 37,87	1,19							
			1,145	709,75	707,75	86156,533	2,148	7,140	86165,821
	14 27,62	1,10							
			1, 05	710,88	708,88	86157,061	1,806	7,115	86165,982
86, 8	26 18, 5	1, 0							
			0,955	711,37	709,37	86157,088	1,491	7,085	86165,664
	38 9,87	0,91							
			0,875	712,50	710,50	86157,473	1,255	7,043	86165,771
86, 6	50 2,37	0,84							
									86165,809
86,73	Mean						Rate of the Clock		+ ,370
									86166,179

April 21, A. M.

Rate of Clock 0″,63 }
Hygrometer 14° dry. }

Barometer { 30,064 / 30,062

Mean - 30,063

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
83,4	17 51 37	1,305							
			1,255	708,62	706,62	86156,489	2,581	5,681	86164,751
	18 3 25,62	1,205							
			1,159	710,13	708,13	86156,664	2,201	5,702	86164,567
83,5	15 15,75	1,114							
			1,082	711,00	709,00	86156,692	1,918	5,732	86164,342
	27 6,75	1,060							
			1,023	712,12	710,12	86157,344	1,715	5,773	86164,832
83,7	38 58,87	0,985							
									86164,623
83,53	Mean						Rate of the Clock		+ ,630
									86165,253

Barometer $\begin{cases} 30{,}062 \\ 30{,}068 \end{cases}$

Mean - 30,065

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
83,7	18 45 38, 5	1,205							
			1,157	709,87	707,87	86156,575	2,193	5,808	86164,576
	57 28,37	1,110							
			1,083	711,26	709,26	86157,050	1,922	5,829	86164,801
83,8	19 9 19,63	1,055							
			1,022	711,74	709,74	86157,214	1,711	5,858	86164,783
	21 11,37	0,990							
			0,953	712,50	710,50	86157,473	1,488	5,901	86164,862
84	33 3,87	0,915							
83,83	Mean							Rate of the Clock	86164,755 + ,630
									86165,385

Hygrometer 14° dry.

Barometer $\begin{cases} 30{,}068 \\ 30{,}072 \end{cases}$

Mean - 30,070

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
84,1	19 40 10	1, 19							
			1, 15	709,63	707,63	86156,492	2,167	5,985	86164,644
	51 59,63	1, 11							
			1,065	713,12	711,12	86157,684	1,858	6,028	86165,570
84,3	20 3 52,75	1, 02							
			0, 98	712, 0	710, 0	86157,303	1,574	6,104	86164,981
	15 44,75	0, 94							
			0,905	713,88	711,88	86157,942	1,342	6,209	86165,493
84,8	27 38,63	0, 87							
84,4	Mean							Rate of the Clock	86165,175 + ,630
									86165,805

April 22, P. M.

Rate of Clock + 0″,76 }
Hygrometer 18°,4 dry. }

Barometer { 30,038
30,025 }

Mean - 30,032

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87,6	2 9 20,37	1,24							
			1,185	708,13	706,13	86155,977	2,301	7,411	86165,689
	21 8,50	1,13							
			1,085	708,37	706,37	86156,059	1,929	7,352	86165,340
87,3	32 56,87	1,04							
			1, 00	709,63	707,63	86156,492	1,638	7,309	86165,439
	44 46,50	0,96							
			0,975	710,87	708,87	86156,917	1,558	7,288	86165,763
87,2	56 37,37	0,89							
87,37	Mean						Rate of the Clock		86165,558 + ,760
									86166,318

Barometer { 30,025
30,017 }

Mean - 30,021

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87,2	3 3 6,37	1,23							
			1, 17	708,25	706,25	86156,018	2,243	7,268	86165,529
	14 54,62	1,11							
			1,065	709,01	707,01	86156,279	1,858	7,246	86165,383
87,1	26 43,63	1,02							
			0, 98	710,75	708,75	86156,876	1,574	7,221	86165,671
	38 34,38	0,94							
			0,895	711,49	709,49	86157,122	1,312	7,204	86165,638
87	50 25,87	0,85							
87,1	Mean						Rate of the Clock		86165,555 + ,760
									86166,315

Hygrometer 16°,2 dry.

Barometer { 29,977 / 29,977

Mean - 29,977

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	"			+	+	
87	4 0 4, 5	1, 30							
			1, 25	707,87	705,87	86155,887	2,560	7,161	86165,608
	11 51,37	1, 20							
			1,155	708	706	86155,932	2,186	7,094	86165,212
86,7	23 39,37	1, 11							
			1,067	709,13	707,13	86156,321	1,865	7,043	86165,229
	35 28, 5	1,025							
			0,993	709,87	707,87	86156,575	1,516	7, 0	86165,091
86,5	47 18,37	0, 96							
86,73	Mean							Rate of the Clock	86165,285 + ,460
									86165,745

April 20, A. M.

Rate of Clock 0",22 + }
Hygrometer 13°,4 }

Barometer { 30,047 / 30,071

Mean - 30,059

	h. m. s.	°	°	"			+	+	
83,3	17 5 11, 5	1,275							
			1,233	711,37	709,37	86157,088	2,491	5,647	86165,226
	18 5 2,87	1, 19							
			1, 15	710,75	708,75	86156,876	2,167	5,689	86164,732
83,5	16 53,62	1, 11							
			1,065	712,13	710,13	86157,347	1,858	5,732	86164,937
	28 45,75	1,020							
			0,990	711,00	709,00	86156,962	1,606	5,774	86164,342
83,7	40 36,75	0, 96							
83,5	Mean							Rate of the Clock	86164,809 + 0,220
									86165,029

Barometer { 30,071
30,088

Mean - 30,079

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	"			+	+	
83,7	18 47 47,75	1, 36							
			1, 31	709,37	707,37	86156,403	2,812	5,829	86165,044
	59 37,12	1, 26							
			1,215	710,51	708,51	86156,794	2,418	5,888	86165,100
84	19 11 27,63	1, 17							
			1, 13	711,87	709,87	86157,259	2,092	5,956	86164,842
	23 19, 5	1, 09							
			1,043	713,25	711,25	86157,728	1,782	6,015	86165,525
84,3	35 12,75	0,995							
84,0	Mean						Rate of the Clock		86165,128
									+ ,022
									86165,150

Hygrometer 13°,4 dry.

Barometer { 30,088
30,082

Mean - 30,085

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	"			+	+	
84,35	19 42 26	1, 39							
			1,337	710, 0	708	86156,619	2,929	6,112	86165,660
	54 16	1,285							
			1,233	710, 5	708, 5	86156,790	2,491	6,197	86165,478
84,75	20 6 65	1,180							
			1,135	711,13	709,13	86157,006	2,111	6,281	86165,398
	17 57,63	1,090							
			1,045	713,24	711,24	86157,725	1,789	6,366	86165,880
85,15	29 50,87	1, 00							
84,75	Mean						Rate of the Clock		86165,604
									+ ,022
									86165,626

April 21, P. M.

Rate of Clock + 0″,37 }
Hygrometer 17°,3 dry. }

Barometer { 30,045 / 30,038
Mean - 30,042

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87,5	2 7 33, 5	1,275							
			1,220	708, 5	706, 5	86156,104	2,439	7,373	86165,916
	19 22	1,165							
			1,123	709,37	707,37	86156,403	2,066	7,305	86165,774
87,2	31 11,37	1,080							
			1,038	710,25	708,25	86156,705	1,765	7,254	86165,724
	43 1,62	0,995							
			0,953	711,01	709,01	86156,965	1,488	7,212	86165,665
87	54 52,63	0,910							
87,23	Mean						Rate of the Clock		86165,770
									+ 0,370
									86166,140

Barometer { 30,038 / 30,030
Mean - 30,034

	h. m. s.	°	°	″			+	+	
87	3 1 43,13	1, 29							
			1, 23	708,49	706,49	86155,959	2,479	7,191	86165,629
	13 31,62	1, 17							
			1,127	709,88	707,88	86156,584	2,081	7,191	86165,856
87	25 21,50	1,085							
			1,043	711, 0	709, 0	86156,962	1,782	7,183	86165,927
	37 12, 5	1, 00							
			0,955	710,88	708,88	86157,061	1,491	7,161	86165,713
86,9	49 3,38	0, 91							
86,93							Rate of the Clock		86165,781
									+ ,370
									86166,151

Hygrometer 17°, 5.

Barometer $\left\{\begin{array}{l}30{,}017\\30{,}007\end{array}\right.$

Mean - 30,012

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87	3 56 31,37	1,14							
			1,085	709,88	707,88	86156,584	1,929	7,170	86165,683
	4 8 21,25	1,03							
			0, 99	711,37	709,37	86157,088	1,606	7,127	86165,821
86,8	20 12,62	0,95							
			0, 91	711,51	709,51	86157,136	1,357	7,106	86165,599
	32 4,13	0,87							
			0, 83	712,75	710,75	86157,558	1,128	7,106	86165,792
86,8	43 56,88	0,79							
									86165,724
86,87	Mean						Rate of the Clock		+ ,760
									86166,484

April 22, A. M.

Barometer $\left\{\begin{array}{l}30{,}022\\30{,}016\end{array}\right.$

Rate of Clock + 0″98
Hygrometer 15° dry (beginning.)

Mean - 30,019

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
83,2	17 45 46	1,26							
			1, 22	714	712	86157,983	2,439	5,596	86166,018
	57 40	1,18							
			1,135	715,62	713,62	86158,531	2,110	5,617	86166,258
83,3	18 9 35,62	1,09							
			1,045	716,88	714,88	86158,955	1,789	5,626	86166,370
	21 32, 5	1, 0							
			0,965	718, 5	716, 5	86159,498	1,526	5,626	86166,650
83,3	33 31	0,93							
									86166,324
83,27	Mean						Rate of the Clock		+ ,980
									86167,304

Hygrometer 14°,2 dry (end.)

Barometer { 30,016
30,040

Mean - 30,028

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
83,4	18 43 3,87	1, 21							
			1, 16	715,63	713,63	86158,534	2,205	5,702	86166,441
	54 59, 5	1,111							
			1,076	717,25	715,25	86159,079	1,897	5,765	86166,741
83,7	19 6 56,75	1, 04							
			1, 00	717,75	715,75	86159,247	1,638	5,829	86166,714
	18 54, 5	0, 96							
			0, 92	718,50	716,50	86159,498	1,387	5,888	86166,773
84	30 53, 0	0, 88							
83,7	Mean						Rate of the Clock		86166,667 + ,980
									86167,647

April 23, P. M.

Rate of Clock + 0″,99 }
Hygrometer 18°,3 dry. }

Barometer { 30,017
30,008

Mean - 30,012

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
	h. m. s.	°	°	″			+	+	
87,6	2 26 10,12	1,25							
			1, 20	707,50	705,50	86155,759	2,359	7,424	86165,542
	37 57,62	1,15							
			1, 10	709,76	707,76	86156,527	1,983	7,381	86165,891
87,4	49 47,38	1,05							
			1,015	709,12	707,12	86156,317	1,688	7,352	86165,357
	3 1 36, 5	0,98							
			0, 94	710, 5	708, 5	86156,790	1,448	7,326	86165,564
87,3	13 27	0,90							
87,43	Mean						Rate of the Clock		86165,588 + 0, 99
									86166,578

Hygrometer 18°,6.

Barometer { 30,008 / 30,000

Mean - 30,004

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations	Observed vibrations in 24 hours.	Correction for Arc.	For Temperature.	Vibrations in 24 hours.
°	h. m. s.	°	°	"			+	+	
87,3	3 20 31	1,21							
			1, 16	717,37	715,37	86155,714	2,205	7,297	86165,216
	32 18,37	1,11							
			1, 07	710,00	708,00	86156,619	1,876	7,254	86165,749
87,1	44 08,37	1,03							
			0, 98	709,38	707,38	86156,407	1,574	7,225	86166,143
	55 57,75	0,93							
			0,895	712,12	710,12	86157,344	1,312	7,216	86165,872
87,05	4 7 49,87	0,86							
87,15	Mean						Rate of the Clock		86165,745 + ,990
									86166,735

Table of the Results of the foregoing Experiments.

SECOND SERIES.

Day. 1821.	Time of the Experiment.	Mean Height of the Thermometer.	Mean Height of the Barometer.	Mean Height of the Hygrometer.	Number of Vibrations in 24 hours, at the temperature of 70° of Farenheit.
		°	Inch.	dry.	
April 18	A. M.	83,05	30,023		86165,857
		84,02	30,037	13,8	86165,951
		85,04	30,044		86166,032
19	P. M.	87,33	29,972		86166,442
		86,93	29,956	16,5	86166,246
		86,43	29,950		86166,382
	A. M.	83, 4	30,044		86165,639
		83,95	30,045	13,5	86165,900
		84,77	30,043		86165,970
20	P. M.	87,23	29,995		86165,473
		87, 0	29,979	17,1	86165,598
		86,73	29,977		86165,745
	A. M.	83, 5	30,059		86165,029
		84, 0	30,079	13,4	86165,150
		84,75	30,085		86165,626
21	P. M.	87,23	30,042		86166,140
		86,93	30,034	14	86166,151
		86,73	30,031		86166,179
	A. M.	83,53	30,063		86165,253
		83,83	30,065	17,4	86165,385
		84, 4	30,070		86165,805
22	P. M.	87,37	30,032		86166,318
		87, 1	30,021	17,3	86166,315
		86,87	30,012		86166,484
	A. M.	83,27	30,019	14,6	86167,304
		83, 7	30,028		86167,647
23	P. M.	87,43	30,012		86166,578
		87,15	30,004	18,4	86166,735
Mean		85,49	30,258	15,6	86166,048

The height of the pendulum above the level of the sea was 27 feet; the distance in a direct line to the sea being about 4900 yards, or 2,784 miles. The country is flat; the nearest elevation being St. Thomas's Mount, which is 9950 yards, or 5,654 miles off, and rises but little above the ordinary level.* There is a range of low hills a short distance beyond St. Thomas's Mount; and the Pulicat Mountains, which are of considerable elevation, are 39 miles off. The soil about Madras is composed of sand and blue mud, and this to as great depths as the wells have been sunk. I do not recollect any rock having been found. I have therefore used 0,66 as a multiplier to 0,095, the correction for 27 feet, which gives 0,06 to be added to the number of beats in 24 hours.

The last correction required was for the buoyancy of the atmosphere. Having no information relative to the specific gravity of the pendulum, I was obliged to determine it in the best way the limited means in this country afforded. This was done with a balance at a dispensary, and with the aid of Mr. Bruce, the proprietor of the establishment. The Madras water drawn from wells in the Black town here, and conducted into the cisterns in the fort, is considered among the purest in the world. This was boiled, and strained into a tin trough prepared for the purpose; the pendulum also was securely and properly slung by means of brass wire, with the assistance of Mr. Gordon, jeweller, of this place. The water was at the same temperature with the atmosphere, and the experiments were made with every care. It may be unnecessary to detail them here; I shall therefore proceed to the result, which was as follows:

* About 150 feet above the level of the sea.

Thermometer 88°, barometer 30,064 inches, specific gravity of the pendulum 8,1085. Hence the specific gravity of the pendulum for the mean of the first series of observations, the thermometer being 83°,48, and barometer 30,121, was 8,02096, and the correction for the buoyancy of the atmosphere is +6,2075 vibrations. For the second series, the thermometer having been 85°, 49, and barometer 30,258 inches, this correction is 6,220 vibrations. These corrections being applied to the number of vibrations before found, will give the true number of vibrations of the pendulum in 24 hours in vacuo at the level of the sea, the thermometer being 70°, and are as follow:—

By the first series of observations, 86172,3755. By the second series, 86172,328. The mean being 86172,352.

The length of the seconds pendulum in London, (latitude 51° 31′ 8″,4 N.) at the temperature of 70°, according to Captain KATER, is 39,142213 inches. Now, the pendulum of experiment used at Madras, made 86293,44 vibrations in 24 hours in London, latitude as before, and 83 feet above the level of the sea, the mean height of the thermometer being 67°, 6, of the barometer 29,97 inches (vide Appendix). The correction for the height above the sea is 0,22, and that for the buoyancy of the atmosphere 6,566, both to be added: these corrections being applied, will give 86300,226 for the number of vibrations of the pendulum of experiment in 24 hours in vacuo at the level of the sea, the temperature being 70°. Now, $86300{,}226^2 : 86400^2 :: 39{,}142213 : 39{,}232772$ the length of the pendulum of experiment.

Then $86172{,}375^2 : 86400^2 :: 39{,}232772 : 39{,}026323087$, the length of the seconds pendulum at Madras by the first series of observations.

Also, $86172{,}328^2 : 86400^2 :: 39{,}232772 : 39{,}026280447$, the length of the seconds pendulum at Madras by the second series.

The mean of both is 39,026302 inches, being, according to Sir GEORGE SHUCKBURGH'S scale, the length of the seconds pendulum by these experiments at Madras in lat. 13° 4′ 9″,1 N. at the level of the sea, in vacuo, and at a temperature of 70° of FAHRENHEIT.

Then comparing this length with 39,142213 inches, the length in latitude 51° 31′ 8,″4 N. as before stated, the diminution of gravity from the pole to the equator will be ,0052894, and the ellipticity $\frac{1}{297{,}56}$ nearly.

J. GOLDINGHAM.

Madras,
May, 1821.

APPENDIX.

The following are the Observations made by Captain KATER *in England, before the Pendulum was sent out.*

July 25, 1820, in lat. 51° 31′ 8″,4.

Clock losing 1ˢ,20 in a mean solar day. Barometer 29,83 Inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
67,4	1 5 25	1,17						
			1,70	1629			1,88	
	32 34	0,97						
			0,90	1634			1,33	
	59 48	0,84						
			0,77	1639			0,97	
	2 27 7	0,71						
			0,65	1642			0,69	
67,8	54 29	0,60						
67,6	Mean			1636	1634	86293,18	1,22	86294,40

July 26.

Clock losing 1ˢ,22. Barometer 30,01 inch.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
66,2	1 37 28	1,28						
			1,18	1621			2,28	
	2 4 29	1,08						
			0,99	1632			1,60	
	31 41	0,91						
			0,84	1637			1,16	
	58 58	0,77						
			0,71	1646			0,83	
66,8	3 26 24	0,65						
66,5	Mean			1634	1632	86293,03	1,47	86294,50

July 27.

Clock losing $1^s,15$. Barometer 30,01 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°				s.	
67,2	1 2 27	1,22						
			1,12	1622			2,06	
	29 29	1,02						
			0,94	1630			1,45	
	56 39	0,86						
			0,79	1633			1,02	
	2 33 52	0,73						
			0,67	1638			0,73	
67,8	51 10	0,62						
67,5	Mean			1630,75	1628,75	86292,89	1,32	86294,21

July 28.

Clock losing 1,05. Barometer 30,01 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
67,8	0 50 41	1,19						
			1,09	1614			1,95	
	1 17 35	1,00						
			0,92	1624			1,39	
	44 39	0,84						
			0,78	1630			1,00	
	2 11 49	0,73						
			0,67	1627			0,73	
68,4	38 56	0,62						
68,1	Mean			1623,75	1621,75	86292,53	1,27	86293,80

July 29.

Clock losing $1^s,07$. Barometer 30,01 inches.

Temp.	Time of coincidence.	Arc of vibration.	Mean Arc.	Interval in seconds.	Number of vibrations.	Observed vibrations in 24 hours.	Correction for Arc.	Vibrations in 24 hours.
°	h. m. s.	°	°					
67,9	0 47 1	1,21						
			1,11	1616			2,02	
	1 13 57	1,02						
			0,94	1620			1,45	
	40 57	0,86						
			0,79	1625			1,03	
	2 8 2	0,73						
			0,68	1630			0,76	
68,8	35 12	0,63						
68,3	Mean			1622,75	1620,75	86292,45	1,31	86293,76

Vibrations of the Pendulum at London.					
Date 1820.	Barometer.	Thermometer.	Vibrations in 24 hours.	Correction for Temperature.	Correct vibrations in a mean solar day at 70°
	Inch.	°			
July 25	29,83	67,6	86294,40	1,02	86293,38
26	30,01	66,5	86294,50	1,48	86293,02
27	30,01	67,5	86294,21	1,06	86293,15
28	30,01	68,1	86293,80	0,70	86293,10
29	30,01	68,3	86293,76	0,72	86293,04
Mean	29,97	67,6			86293,14

From the above table it appears, that the pendulum makes 86293,14 vibrations in a mean solar day, in latitude 51° 31′ 8″,4 the temperature being 70°, and the height above the level of the sea 83 feet. The correction employed for temperature is 0,423 of a vibration for one degree. In computing the correction for the buoyancy of the atmosphere during the experiments, the temperature of 67°,6 must be used, the barometer being at 29,97 inches.

J. GOLDINGHAM.

XVI. *Account of an assemblage of Fossil Teeth and Bones of Elephant, Rhinoceros, Hippopotamus, Bear, Tiger, and Hyæna, and sixteen other animals; discovered in a cave at Kirkdale, Yorkshire, in the year* 1821: *with a comparative view of five similar caverns in various parts of England, and others on the Continent. By the Rev.* WILLIAM BUCKLAND, *F. R. S. F. L. S. Vice President of the Geological Society of London, and Professor of Mineralogy and Geology in the University of Oxford, &c. &c. &c.*

Read February 21, 1822.

HAVING been induced in December last to visit Yorkshire, for the purpose of investigating the circumstances of the cave at Kirkdale, near Kirby Moorside, about 25 miles N. N. E. of the city of York, in which a discovery was made last summer of a singular collection of teeth and bones, I beg to lay before the Royal Society the result of my observations on this new and interesting case, and to point out some important general conclusions that arise from it.

The facts I have collected, seem calculated to throw an important light on the state of our planet at a period antecedent to the last great convulsion that has affected its surface; and I may add, in limine, that they afford one of the most complete and satisfactory chains of consistent circumstantial evidence I have ever met with in the course of my geological investigations.

As I shall have frequent occasion to make use of the word

diluvium, it may be necessary to premise, that I apply it to those extensive and general deposits of superficial gravel, which appear to have been produced by the last great convulsion that has affected our planet; and that with regard to the indications afforded by geology of such a convulsion, I entirely coincide with the views of M. CUVIER, in considering them as bearing undeniable evidence of a recent and transient inundation.* On these grounds I have felt myself fully justified in applying the epithet *diluvial*, to the results of this great convulsion, of *antediluvial*, to the state of things immediately preceding it, and *postdiluvial*, or *alluvial*, to that which succeeded it, and has continued to the present time.

In detailing these observations I propose, first, to submit a short account of the geological position and relations of the rock in which the cavern alluded to is situated; to proceed, in the next place, to a description of the cavern itself; then to enter into that which will form the most important part of this communication, a particular enumeration of the animal remains there inhumed, and the very remarkable phenomena with which they are attended; to review the general inferences to which these phenomena lead; and conclude with a brief comparative account of analogous animal deposits in other parts of this country, and the Continent.

Kirkdale is situated (as may be seen by reference to the

* Analogous evidences to the same point, collected in this country from the state of the gravel beds and vallies in the midland parts of England, have recently been published by myself in a Paper on the Lickey Hill, in the second part of the fifth Volume of the Geological Transactions, and in the Appendix to an Inaugural Lecture I published at Oxford, in 1820. Another Paper of mine on similar evidences afforded by the vallies that intersect the coast of West Dorset and East Devonshire, will be published in the first part of the sixth Volume of the Geological Transactions.

annexed map, Plate XV.) about 25 miles N.N.E. of the city of York, between Helmsley and Kirby Moorside, near the point at which the east base of the Hambleton hills, looking towards Scarborough, subsides into the vale of Pickering, and on the S. extremity of the mountainous district known by the name of the Eastern and the Cleveland Moorlands.

The substratum of this valley of Pickering is a mass of stratified blue clay, identical with that which at Oxford and Weymouth reposes on a similar lime-stone to that of Kirkdale, and containing, subordinately, beds of inflammable bituminous shale, like that of Kimeridge, in Dorsetshire. Its south boundary is formed by the Howardian hills, and by the elevated escarpment of the chalk that terminates the Wolds towards Scarborough. Its north frontier is composed of a belt of lime-stone, extending eastward 30 miles from the Hambleton hills, near Helmsley, to the sea at Scarborough, and varying in breadth from 4 to 7 miles; this lime-stone is intersected by a succession of deep and parallel vallies, (here called dales) through which the following rivers, from the moorlands, pass down southwards to the vale of Pickering, viz. the Rye, the Rical, the Hodge Beck, the Dove, the Seven Beck, and the Costa; their united streams fall into the Derwent above New Malton, and their only outlet is by a deep gorge, extending from near this town down to Kirkham, the stoppage of which would at once convert the whole vale of Pickering into an immense inland lake; and before the excavation of which, it is probable, that such a lake existed, having its north border nearly along the edge of the belt of lime-stone just described, and at no great distance from the mouth of the cave at Kirkdale.

The position of the cave is at the south and lower extremity of one of these dales (that of the Rical Beck), at the point where it falls into the vale of Pickering, at the distance of about a furlong from the church of Kirkdale, and near the brow of the left flank of the valley, close to the road. This flank slopes towards the river at an angle of 25°, and the height of the brow of the slope above the water may be about 120 feet. (See Plate XVI. fig. 1.)

The rock perforated by the cave is referable to that portion of the oolite formation which, in the south of England, is known by the name of the Oxford oolite and coral rag: its organic remains are identical with those of the Heddington quarries near Oxford, but its substance is harder and more compact, and more interspersed with siliceous matter, forming irregular concretions, beds, and nodules of chert in the lime-stone, and sometimes entirely penetrating its coralline remains. The most compact beds of this lime-stone resemble the younger alpine lime-stone of Meillierie and Aigle, in Switzerland, and they alternate with and pass gradually into those of a coarser oolitic texture; and both varieties are stratified in beds from one to four feet thick. The cave is situated in one of the compact beds which lies between two others of the coarser oolitic variety; the latter vary in colour from light yellow to blue; the compact beds are of a dark grey passing to black, are extremely fetid, and full of corals and spines of the echinus cidaris. The compact portions of this oolite partake of the property common to compact lime-stones of all ages and formations, of being perforated by irregular holes and caverns intersecting them in all directions; the cause of these cavities has never been satisfactorily

ascertained: into this question (which is one of considerable difficulty in geology) it is foreign to my present purpose to enquire, any farther than to state that they were neither produced, enlarged, or diminished by the presence of the animals whose bones we now find in them.

The abundance of such caverns in the lime-stone of the vicinity of Kirkdale, is evident from the fact of the engulfment of several of the rivers above enumerated in the course of their passage across it from the eastern moorlands to the vale of Pickering; and it is important to observe, that the elevation of the Kirkdale cave, above the bed of the Hodge Beck, exceeding 100 feet, excludes the possibility of our attributing the muddy sediment we shall find it to contain, to any land flood or extraordinary rise of the waters of that or any other now existing river.

It was not till the summer of 1821, that the existence of any animal remains, or of the cavern containing them, had been suspected. At this time, in continuing the operations of a large quarry along the brow of the slope just mentioned, (see Plate XVI. fig. 1.) the workmen accidentally intersected the mouth of a long hole or cavern, closed externally with rubbish, and overgrown with grass and bushes. As this rubbish was removed before any competent person had examined it, it is not certain whether it was composed of diluvial gravel and rolled pebbles, or was simply the debris that had fallen from the softer portions of the strata that lay above it; the workmen, however, who removed it, and some gentlemen who saw it, assured me, that it was composed of gravel and sand. In the interior of the cave there was not a single rolled pebble, nor one bone, or fragment of bone, that bears the

slightest mark of having been rolled by the action of water. A few bits of lime-stone and roundish concretions of chert that had fallen from the roof and sides, were the only rocky fragments that occurred, with the exception of stalactite.

About 30 feet of the outer extremity of the cave have now been removed, and the present entrance is a hole in the perpendicular face of the quarry less than 5 feet square, which it is only possible for a man to enter on his hands and knees, and which expands and contracts itself irregularly from 2 to 7 feet in breadth and height, diminishing however as it proceeds into the interior of the hill. The cave is about 15 or 20 feet below the incumbent field, the surface of which is nearly level, and parallel to the stratification of the lime-stone, and to the bottom of the cave. Its main direction is E. S. E. but deviating from a straight line by several zigzags to the right and left (see Pl. XVI. fig. 3.); its greatest length is from 150 to 200 feet. In its interior it divides into several smaller passages, the extent of which has not been ascertained. In its course it is intersected by some vertical fissures, one of which is curvilinear, and again returns to the cave; another has never been traced to its termination; whilst the outer extremity of a third, is probably seen in a crevice or fissure that appears on the face of the quarry, and which closes upwards before it leaves the body of the lime-stone. By removing the sediment and stalactite that now obstruct the smaller passages, a farther advance in them may be rendered practicable. The half corroded fragments of corals, of spines of echini and other organic remains, and the curious ledges of lime-stone and nodules of chert that project along the sides and roof of the cave, together with the small

grooves and pits that cover great part of its interior, show that there was a time when its dimensions were less than at present; though they fail to prove by what cause it was originally produced. There are but two or three places in which it is possible to stand upright, and these are where the cavern is intersected by the fissures; the latter of which continue open upwards to the height only of a few feet, when they gradually close, and terminate in the body of the lime-stone: they are thickly lined with stalactite, and are attended by no fault or slip of either of their sides. Both the roof and floor, for many yards from the entrance, are composed of horizontal strata of lime-stone, uninterrupted by the slightest appearance of fissure, fracture, or stony rubbish of any kind; but farther in, the roof and sides become irregularly arched, presenting a very rugged and grotesque appearance, and being studded with pendent and roundish masses of chert and stalactite; the bottom of the cavern is visible only near the entrance; and its irregularities, though apparently not great, have been filled up throughout to a nearly level surface, by the introduction of a bed of mud or sediment, the history of which, and also of the stalactite, I shall presently describe. (See Plate XVI. fig. 2).

The fact already mentioned of the engulphment of the Rical Beck, and other adjacent rivers, as they cross the lime-stone, showing it to abound with many similar cavities to those at Kirkdale, renders it likely that, hereafter, similar deposits of bones may be discovered in this same neighbourhood; but accident alone can lead to such discovery, as it is probable the mouths of these caverns are buried under diluvian sand and gravel, or post-diluvian detritus; so that nothing but their

casual intersection by some artificial operations, will lead to the knowledge of their existence; and in this circumstance, we also see a reason why so few caverns of this kind have hitherto been discovered, although it is probable that many such may exist.

In all these cases, the bones found in caverns are never mineralised, but simply in the state of grave bones, or incrusted by stalactite; and have no farther connection with the rocks themselves, than that arising from the accident of having been lodged in their cavities, at periods long subsequent to the formation and consolidation of the strata in which these cavities occur.

On entering the cave at Kirkdale (see Plate XVI. fig. 2), the first thing we observe is a sediment of mud, covering entirely its whole bottom to the average depth of about a foot, and entirely covering and concealing the subjacent rock, or actual floor of the cavern. Not a particle of mud is found attached either to the sides or roof; nor is there a trace of it adhering to the sides or upper portions of the transverse fissures, or any thing to suggest the idea that it entered through them. The surface of this sediment when the cave was first entered was nearly smooth and level, except in those parts where its regularity had been broken by the accumulation of stalagmite above it, or ruffled by the dripping of water: its substance is argillaceous and slightly micaceous loam, composed of such minute particles as would easily be suspended in muddy water, and mixt with much calcareous matter, that seems to have been derived in part from the dripping of the roof, and in part from comminuted bones.

Above this mud, on advancing some way into the cave, the

roof and sides are seen to be partially studded and cased over with a coating of stalactite, which is most abundant in those parts where the transverse fissures occur, but in small quantity where the rock is compact and devoid of fissures. Thus far it resembles the stalactite of ordinary caverns; but on tracing it downwards to the surface of the mud, it was there found to turn off at right angles from the sides of the cave, and form above the mud a plate or crust, shooting across like ice on the surface of water, or cream on a pan of milk. (See Plate XVI. fig. 2). The thickness and quantity of this crust varied with that found on the roof and sides, being most abundant, and covering the mud entirely where there was much stalactite on the sides, and more scanty in those places where the roof presented but little: in many parts it was totally wanting both on the roof and surface of the mud and subjacent floor. Great portion of this crust had been destroyed in digging up the mud to extract the bones; it still remained, however, projecting partially in some few places along the sides; and in one or two, where it was very thick, it formed, when I visited the cave, a continuous bridge over the mud entirely across from one side to the other. In the outer portion of the cave, there was a mass of this kind which had been accumulated so high as to obstruct the passage, so that a man could not enter till it had been dug away.

These horizontal incrustations have been formed by the water which, trickling down the sides, was forced to ooze off laterally as soon as it came into contact with the mud; in other parts, where it fell in drops from the roof, stalagmitic accumulations have been raised on its surface, some of which are very large, but more commonly they are of the size and

shape of a cow's pap, a name which the workmen have applied to them. There is no alternation of mud with any repeated beds of stalactite, but simply a partial deposit of the latter on the floor beneath it; and it was chiefly in the lower part of the sediment above described, and in the stalagmitic matter beneath it, that the animal remains were found: its substance contains no black earth or admixture of animal matter, except an infinity of extremely minute particles of undecomposed bone. In the whole extent of the cave, only a very few large bones have been discovered that are tolerably perfect; most of them are broken into small angular fragments and chips, the greater part of which lay separately in the mud, whilst others were wholly or partially invested with stalactite; and some of the latter united with masses of still smaller fragments and cemented by the stalactite, so as to form an osseous breccia, of which I have specimens.

The effect of this mud in preserving the bones from decomposition has been very remarkable; some that had lain a long time before its introduction were in various stages of decomposition; but even in these, the farther progress of decay appears to have been arrested by it; and in the greater number, little or no destruction of their form, and scarcely any of their substance, has taken place. I have found on immersing fragments of these bones in an acid till the phosphate and carbonate of lime were removed, that nearly the whole of their original gelatine has been preserved. Analogous cases of the preservative powers of diluvial mud occur on the coast of Essex, near Walton, and at Lawford, near Rugby, in Warwickshire. Here the bones of the same species of elephant, rhinoceros, and other diluvial animals occur in a state of freshness and freedom from decay, nearly equal

to those in the cave at Kirkdale, and this from the same cause, viz. their having been protected from the access of atmospheric air, or the percolation of water, by the argillaceous matrix in which they have been imbedded: whilst similar bones that have lain the same length of time in diluvial sand, or gravel, and been subject to the constant percolation of water, have lost their compactness and strength and great part of their gelatine, and are often ready to fall to pieces on the slightest touch; and this where beds of clay and gravel occur alternating in the same quarry, as at Lawford.

The workmen on first discovering the bones at Kirkdale, supposed them to have belonged to cattle that died by a murrain in this district a few years ago, and they were for some time neglected, and thrown on the roads with the common lime-stone; they were at length noticed by Mr. Harrison, a medical gentleman of Kirby Moorside, and have since been collected and dispersed amongst so many individuals, that it is probable nearly all the specimens will in a few years be lost, with the exception of such as may be deposited in public collections. By the kindness and liberality of the Bishop of Oxford (to whom I am also indebted for my first information of the discovery of this cave) and of C. Duncombe, Esq. and Lady Charlotte Duncombe, of Duncombe Park, a nearly complete series of the teeth of all these animals has been presented to the Museum at Oxford; whilst a still better collection both of teeth and bones is in the possession of J. Gibson, Esq. of Stratford in Essex, to whose exertions we owe the preservation of many valuable specimens, and who is about to present a series of them to our public collections in London. W. Salmond, Esq. also, since I visited Kirkdale in December last, has been engaged with much zeal and acti-

vity in measuring and exploring new branches of the cave, and making large collections of the teeth and bones, from which I understand he also intends to enrich our public cabinets in the metropolis. I am indebted to him for the annexed ground plan of the cave, and its ramifications, (Plate XVI. fig. 3). Drawings by Mr. CLIFT, of some of the most perfect of Mr. GIBSON'S specimens, have been sent to M. CUVIER, for the new edition of his work on fossil animals; copies of these have been made for me by Miss MORLAND, and appear in the annexed plates, with many other drawings, for which I am indebted to the pencil of Miss DUNCOMBE; and the Rev. GEORGE YOUNG, and Mr. BIRD of Whitby, in their History of the Geology of the coast of Yorkshire, have given engravings of tome teeth that remain in their possession.

It appears that the teeth and bones which have as yet been discovered in the cave at Kirkdale, are referable to the following twenty-two species of animals.

7 Carnivora. Hyæna, Tiger, Bear, Wolf, Fox, Weasel, and an unknown animal of the size of a Wolf. (See Plates XVII. XVIII. XIX. XX.)

4 Pachydermata. Elephant, Rhinoceros, Hippopotamus and Horse. (See Plate XXI.)

4 Ruminantia. Ox, and three species of Deer. (See Plates XXII. XXIII.)

3 Rodentia. Rabbit, Water-rat and Mouse. (See Plate XXIV. XXV.)

4 Birds. Raven, Pigeon, Lark, and a small species of Duck, resembling the anas sponsor, or summer Duck. (See Plate XXV.)

The bottom of the cave, on first removing the mud, was

found to be strewed all over like a dog kennel, from one end to the other, with hundreds of teeth and bones, or rather broken and splintered fragments of bones, of all the animals above enumerated; they were found in greatest quantity near its mouth, simply because its area in this part was most capacious; those of the larger animals, elephant, rhinoceros, &c. were found co-extensively with all the rest, even in the inmost and smallest recesses, (see Plate XVI. fig. 3). Scarcely a single bone has escaped fracture, with the exception of the astragalus, and other hard and solid bones of the tarsus and carpus joints, and of the toes (see Plate XXIV. fig. 1 to 5, and fig. 7 to 10, and Plate XIX. fig. 5 to 12). On some of the bones marks may be traced, which, on applying one to the other, appear exactly to fit the form of the canine teeth of the hyæna that occur in the cave. The hyænas' bones have been broken, and apparently gnawed equally with those of the other animals. Heaps of small splinters, and highly comminuted, yet angular fragments of bone, mixed with teeth of all the varieties of animals above enumerated, lay in the bottom of the den, occasionally adhering together by stalactite, and forming, as has been before mentioned, an osseous breccia. Many insulated fragments also are wholly or partially enveloped with stalactite, both externally and internally. Not one skull is to be found entire; and it is so rare to find a large bone of any kind that has not been more or less broken, that there is no hope of obtaining materials for the construction of any thing like a skeleton. The jaw bones also, even of the hyænas, are broken like the rest; and in the case of all the animals, the number of teeth and of solid bones of the tarsus and carpus,

is more than twenty times as great as could have been supplied by the individuals whose other bones we find mixed with them.

Fragments of jaw bones are by no means common; the greatest number I saw belong to the deer, hyæna, and water-rat, and retain their teeth; in all the jaws both teeth and bone are in an equal state of high preservation, and show that their fracture has been the effect of violence, and not of natural decay. I have seen but ten fragments of deers' jaws, and about forty of hyænas', and as many of rats. (See Plate XVIII. fig. 2, 3, and Plate XVII. fig. 3, 4, 5.) The ordinary fate of the jaw bones, as of all the rest, appears to have been to be broken to pieces.

The greatest number of teeth are those of hyænas, and the ruminantia. Mr. GIBSON alone collected more than 300 canine teeth of the hyæna, which at the least must have belonged to 75 individuals, and they are in the same proportion in other collections. The only remains that have been found of the tiger species (see Plate XX. fig. 5, 6, 7), are two large canine teeth, each 4 inches in length, and one molar tooth, exceeding in size that of the largest lion or Bengal tiger. There is one tusk only of a bear (see Plate XX. fig. 1), which exactly resembles those of the extinct ursus spelæus of the caves of Germany, the size of which M. CUVIER says must have equalled that of a large horse. Of the wolf and fox there are many teeth (see Plate XX. fig. 8 to 18), and others belonging to an animal which I cannot ascertain: it seems to have been nearly allied to the wolf, but the teeth are much tninner, and less strong. (See Plate XX. fig. 20 to 27). A few jaws and teeth have also been found belonging to the

weasel. (Plate XX. fig. 28, 29.) Teeth of the larger pachydermatous animals are not abundant. I have information of about ten elephants' teeth, but of no tusk; and as very few of these teeth exceed three inches in their longest diameter, they must have belonged to very young animals. (See Plate XXI. fig. 1 and 2). I have seen but six molar teeth of the hippopotamus, and a few fragments of its canine and incisor teeth; some of which latter are in the possession of Mr. THORPE, of York. Teeth of the rhinoceros are not so rare. I have seen 40 or 50, and some of them extremely large ones, and apparently from aged animals. I have heard of only two or three teeth belonging to the horse. Of the teeth of deer there are at least three species (see Plate XXII. fig. 9,11,13), the smallest being very nearly of the size and form of those of a fallow deer, the largest agreeing in size, but differing in form from those of the modern elk; and a third being of an intermediate size, and approaching that of a large stag or red deer. I have not ascertained how many species there are of ox, but apparently there are at least two. But the teeth which occur perhaps in greatest abundance, are those of the water-rat (see Pl. XXV. fig. 1 to 5, and 11 to 18;) for in almost every specimen I have collected or seen of the osseous breccia, there are teeth or broken fragments of the bones of this little animal mixed with and adhering to the fragments of all the larger bones. These rats may be supposed to have abounded on the edge of the lake, which I have shown probably to have existed at that time in this neighbourhood: there are also a few teeth and bones of rabbits and mice. (Plate XXIV. fig. 14, 15, 16, 17, 18, and Plate XXV. fig. 7, 8, 9, 10).

Besides the teeth and bones already described, the cave

contained also remains of horns of at least two species of deer, (see Plate XXIII. fig. 3, 4, and 5.) One of these resembles the horn of the common stag or red deer, the circumference of the base measuring $9\frac{3}{4}$ inches, which is precisely the size of our largest stag. A second (fig. 4.) measures $7\frac{3}{4}$ inches at the same part, and both have two antlers, that rise very near the base. In a smaller species the lowest antler is $3\frac{1}{2}$ inches above the base, the circumference of which is 8 inches, (see fig. 5.) No horns are found entire, but fragments only, and these apparently gnawed to pieces like the bones: their lower extremity nearest the head is that which has generally escaped destruction: and it is a curious fact, that this portion of all the horns I have seen from the cave, shows, by the rounded state of the base, that they had fallen off by absorption or necrosis, and been shed from the head on which they grew, and not broken off by violence.

It must already appear probable, from the facts above described, particularly from the comminuted state and apparently gnawed condition of the bones, that the cave at Kirkdale was, during a long succession of years, inhabited as a den by hyænas, and that they dragged into its recesses the other animal bodies whose remains are found mixed indiscriminately with their own; and this conjecture is rendered almost certain by the discovery I made, of many small balls of the solid calcareous excrement of an animal that had fed on bones, resembling the substance known in the old Materia Medica by the name of album græcum (see Plate XXIV. fig. 6.): its external form is that of a sphere, irregularly compressed, as in the fæces of sheep, and varying from half an inch to an inch in diameter; its colour is yellowish white, its fracture

is usually earthy and compact, resembling steatite, and sometimes granular; when compact, it is interspersed with minute cellular cavities: it was at first sight recognised by the keeper of the Menagerie at Exeter Change, as resembling, both in form and appearance, the fæces of the spotted or Cape Hyæna, which he stated to be greedy of bones, beyond all other beasts under his care. This information I owe to Dr. WOLLASTON, who has also made an analysis of the substance under discussion, and finds it to be composed of the ingredients that might be expected in fæcal matter derived from bones, viz. phosphate of lime, carbonate of lime, and a very small proportion of the triple phosphate of ammonia and magnesia; it retains no animal matter, and its originally earthy nature and affinity to bone, will account for its perfect state of preservation.

I do not know what more conclusive evidence than this can be added to the facts already enumerated, to show that the hyænas inhabited this cave, and were the agents by which the teeth and bones of the other animals were there collected; it may be useful therefore to consider, in this part of our enquiry, what are the habits of modern hyænas, and how far they illustrate the case before us.

The modern hyæna (of which there are only three known species, all of them smaller and different from the fossil one) is an inhabitant exclusively of hot climates; the most savage, or striped species, abounds in Abyssinia, Nubia, and the adjacent parts of Africa and Asia. The less ferocious, or spotted one, inhabits the Cape of Good Hope, and lives principally on carrion. In bony structure the latter approaches more nearly than the former to the fossil species: to these M. CUVIER adds a third, the red hyæna, which is very rare.

The structure of these animals places them in an intermediate class between the cat and dog tribes; not feeding, like the former, almost exclusively on living prey, but like the latter, being greedy also of putrid flesh and bones:* their love of putrid flesh induces them to follow armies, and dig up human bodies from the grave. They inhabit holes which they dig in the earth, and chasms of rocks; are fierce, and of obstinate courage, attacking stronger quadrupeds than themselves, and even repelling lions. Their habit of digging human bodies from the grave, and dragging them to their den, and of accumulating around it the bones of all kinds of animals, is thus described by BUSBEQUIUS, where he is speaking of the Turkish mode of burial in Anatolia, and their custom of laying large stones upon their graves to protect them from the Hyænas. "Hyæna regionibus iis satis frequens; sepulchra suffodit, extrahitque cadavera, portatque ad suam speluncam; juxta quam videre est ingentem cumulum ossium humanorum 'veterinariorum'† et reliquorum omne genus animalium." (Busbeq. Epist. 1. Leg. Turc.) BROWN, also, in his Travels to Darfur, describes the Hyænas' manner of taking off their prey in the following words:—"they come in herds of six, eight, and often more, into the villages at night, and carry off with them whatever they are able to master; they will kill dogs and asses even within the enclosure of houses, and fail not to assemble wherever a dead camel or

* It is quite impossible to mistake the jaw of any species of hyæna for that of the wolf or tiger kind; the latter having three molar teeth only in the lower jaw, and the former seven; whilst all the hyæna tribe have four. (See Plate XVIII. fig. 1, 2, 3.)

† Veterinam bestiam jumentum CATO appellavit a vehendo: (quasi veheterinus vel veterinus.) Pomp. Fest.

other animal is thrown, which, acting in concert, they sometimes drag to a prodigious distance." SPARMAN and PENNANT mention that a single hyæna has been known to carry off a living man or woman in the vicinity of the Cape.

The strength of the hyæna's jaw is such, that in attacking a dog, he begins by biting off his leg at a single snap. The capacity of his teeth, for such an operation, is sufficiently obvious from simple inspection, and had long ago attracted the attention of the early naturalists; and, consistent with this strength of teeth and jaw, is the state of the muscles of his neck, being so full and strong, that in early times this animal was fabled to have but one cervical vertebra. They live by day in dens, and seek their prey by night, having large prominent eyes, adapted, like those of the rat and mouse, for seeing in the dark. To animals of such a class, our cave at Kirkdale would afford a most convenient habitation, and the circumstances we find developed in it are entirely consistent with the habits above enumerated.

It appears from the researches of M. CUVIER, that the fossil hyæna was nearly one third larger than the largest of the modern species, that is, the striped or Abyssinian; but, in the structure of its teeth, more nearly resembled that of the Cape animal. (See Plate XVIII. fig. 1, 2, 3.) Its muzzle also was shorter and stronger than in either of them, and consequently its bite more powerful. The length of the largest modern hyæna noticed is 5 feet 9 inches.

The fossil species has been found on the Continent in situations of two kinds, both of them consistent with the circumstances under which it occurs in Yorkshire, and, on comparing the jaws and teeth of the latter with those of the former

engraved in M. CUVIER'S *Recherches sur les ossements fossiles*, I find them to be absolutely identical. The two situations are caverns and diluvian gravel.

1. In Franconia, a few bones of hyæna were found mixt with those of an enormous number of bears, in the cave of Gailenreuth.
2. At Muggendorf, in a similar cave.
3. At Bauman, in ditto.
4. At Fouvent, near Gray, in the department of Doubes, bones of hyæna were found mixt with those of the elephant and horse in a fissure of lime-stone rock, which, like that at Kirkdale, was discovered by the accidental digging away of the rock in a garden.
5. At Canstadt, in the valley of the Necker, A. D. 1700, hyænas' bones were found mixt with those of the elephant, rhinoceros, and horse, and with rolled pebbles, in a mass of yellowish clay.
6. Between Hahldorf and Reiterbuck, on the surface of the hills that bound the valley of Eichstadt in Bavaria. These were buried in a bed of sand.

The four first of these cases appear to have been dens, like the cave at Kirkdale; the two latter are deposits of diluvian detritus, like the surface gravel beds of England, in which similar remains of all the other animals have been found, excepting hyænas.

It has been observed when speaking of the den, that the bones of the hyænas are as much broken to pieces as those of the animals that formed their prey; and hence we must infer, that the carcases even of the hyænas themselves, were eaten up by their survivors. Whether it be the habit of modern

hyænas to devour those of their own species that die in the course of nature; or under the pressure of extreme hunger to kill and eat the weaker of them, is a point on which it is not easy to obtain positive evidence. Mr. Brown however asserts, in his journey to Darfur, "that it is related of the hyænas, that upon one of them being wounded, his companions instantly tear him to pieces and devour him." It seems therefore in the highest degree probable, that the mangled relics of hyænas that lie indiscriminately scattered and equally broken with the bones of other animals in the cave of Kirkdale, were reduced to this state by the agency of the surviving individuals of their own species.

A large proportion of the hyænas' teeth bear marks of extreme old age, some being abraded to the very sockets, and the majority having lost the upper portion of their coronary part, and having fangs extremely large: these probably died in the den from mere old age: and if we compare the lacerated condition of the bones that accompany them, with the state of the teeth thus worn down to the very stumps, notwithstanding their prodigious strength, we find in the latter the obvious instruments by which the former were thus comminuted. A great number of other teeth appear to have belonged to young hyænas, for the fangs are not developed, and the points and edges of the crown not the least worn down. I have a fragment of the jaw of an hyæna which died so young, that the second set of its teeth had not been protruded, but were in the act of forming within the jaw. (See Plate XIX. fig. 3, 4.) Others are in various stages of advancement towards maturity; and the proportion of these is too great for us to attribute them to animals that may have

died in early life from accident or disease. It seems more probable, and the idea is confirmed by the above statement of Mr. BROWN, and by the fact of the hyænas' bones in the den being gnawed and broken to pieces equally with the rest, that they were occasionally killed and devoured by the stronger individuals of their own species.

But, besides the evidence their teeth afford to show that the animals died at various periods of life, they present other appearances (and so likewise do the bones), of having passed through different stages and gradations of decay, arising from the different length of time they had lain exposed in the bottom of the den, before the muddy sediment entered, which, since its introduction, has preserved them from farther decomposition. This observation applies equally to all the animals. I have portions of bone and teeth that are so much decomposed as to be ready to fall to pieces by the slightest touch; these had probably lain a long time unprotected in the bottom of the den; others still older may have entirely perished; but the majority both of teeth and fragments of bone are in a state of the highest preservation; and many thousands have been collected and carried away since the cave was discovered. In all cases the degree of decay is equal in the teeth and jaw bones, or fragments of jaws, to which they are attached.

In many of the most highly preserved bones and teeth, there is a curious circumstance, which, before I visited Kirkdale, had convinced me of the existence of the den, viz. a partial polish and wearing away to a considerable depth of one side only; many straight fragments of the larger bones have one entire side, or the fractured edges of one side rubbed

down and worn completely smooth, whilst the opposite side and ends of the same bone are sharp and untouched; in the same manner as the upper portions of pitching stones in the street become rounded and polished, whilst their lower parts retain the exact form and angles which they possessed when first laid down. This can only be explained by referring the partial destruction of the solid bone to friction from the continual treading of the hyænas, and rubbing of their skin on the side that lay uppermost in the bottom of the den. In many of the smaller and curved bones, also, particularly in those of the lower jaw, (see Plate XIX. fig. 1 and 2.) the convex surface only is uniformly that which has been worn down and polished, whilst the ends and concave surface have suffered no kind of change or destruction, (Plate XIX. fig. 3 and 4.): and this also admits of a similar explanation; for the curvature of the bone would allow it to rest steady under constant treading only in this position; as long as the concave surface was uppermost, pressure on either extremity would cause it to tilt over and throw the convex side upwards; and this done, the next pressure would cause its two extremities to sink into any soft substance that lay beneath, and give it a steady and fixed position. Such seems to have been the process by which the curved fragments I allude to, have not only received a partial polish on the convex side only, but have been submitted to so much friction, that in several instances more than one-fourth of the entire thickness of the bone, and a proportionate quantity of the outer side of the fangs and body of the teeth, have been entirely worn away. (See Plate XIX. fig. 1.) I can imagine no other means than the repeated touch of the living hyænas' feet and skin, by

which this partial wearing away and polish can have been produced:* for the process of rolling by water would have made pebbles of them, or at least would have broken off the edges of the teeth and delicate points of the fractured extremities of the bone, which still remain untouched and sharp.

I have already stated, that the greatest number of teeth (those of the hyæna excepted) belong to the ruminating animals; from which it is to be inferred that they formed the ordinary prey of the hyænas. I have also to add, that very few of the teeth of these animals bear marks of age; they seem to have perished by a violent death in the vigour of life. With respect to the horns of deer that appear to have fallen off by necrosis, it is probable that the hyænas found them thus shed, and dragged them home for the purpose of gnawing them in their den; and to animals so fond of bones, the spongy interior of horns of this kind would not be unacceptable. I found a fragment of stags' horn in so small a recess of the cave, that it never could have been introduced, unless singly, and after separation from the head; and near it was the molar tooth of an elephant. I have seen no remains of horns of oxen, and perhaps there are none, for the bony portion of their interior being of a porous spongy nature, would probably have been eaten by the hyænas, whilst the outer case, being of a similar composition to hair and hoofs,

* I have been informed by an officer in India, that passing by a tiger's den in the absence of the tiger, he examined the interior, and found in the middle of it a large portion of stone on which the tiger reposed, to be worn smooth and polished by the friction of his body. The same thing may be seen on marble steps and altars, and even metallic statues in places of worship that are favourite objects of pilgrimage: they are often deeply worn and polished by the knees, and even lips of pilgrims, to a degree that, without experience of the fact, we could scarcely have anticipated.

would not long have escaped total decomposition. For the same reason the horn of the rhinoceros, being merely a mass of compacted hair-like fibres, has never been found fossil in gravel beds with the bones of that animal, nor does it occur in the cave at Kirkdale. I have been told that sheeps' horns laid on land for manure will be consumed in ten or a dozen years; the calcareous matter of bone being nearly allied to lime-stone, is the only portion of animal bodies that occurs in a fossil state, unless when preserved, like the Siberian elephant, of the same extinct species with that of Kirkdale, by being frozen in ice, or buried in peat.

The extreme abundance of the teeth of water rats has also been alluded to; and though the idea of hyænas eating rats may appear ridiculous, it is consistent with the omnivorous appetite of modern hyænas; nor is the disproportion in size of the animal to that of its prey, greater than that of wolves and foxes, which are supposed by Captain Parry to feed chiefly on mice during the long winters of Melville Island. Our largest dogs eat rats and mice; jackalls occasionally prey on mice, and dogs and foxes will eat frogs. It is probable, therefore, that neither the size nor aquatic habit of the water rat would secure it from the hyænas. They might occasionally also have eaten mice, weasels, rabbits, foxes, wolves, and birds; and in masticating the bodies of these small animals with their coarse conical teeth, many bones and fragments of bone would be pressed outwards through their lips, and fall neglected to the ground.

The occurrence of birds' bones may be explained by the probability of the hyænas finding them dead, and taking them home, as usual, to eat in their den: and the fact, that four

of the only five bones of birds I have seen from Kirkdale are those of the ulna, may have arisen from the position of the quill feathers on it, and the small quantity of fleshy matter that exists on the outer extremity of the wing of birds; the former affording an obstacle, and the latter no temptation to the hyænas to devour them. Two of the five bones here mentioned (see Plate XXV. fig. 19 to 29), in size and form, and the position of the points at the base of the quills, exactly resemble the ulna of a raven; a third approaches as closely to the Spanish runt, which is one of the largest of the pigeon tribe; a fourth bone is the right ulna of a lark; and a fifth, the coracoid process of the right scapula of a small species of duck resembling the Anas sponsor, or summer duck.*

With respect to the bear and tiger, the remains of which are extremely rare, and of which the teeth that have been found (see Plate XX. fig. 1, 5, 6, and 7), indicate a magnitude equal to the great Ursus spelæus of the caves of Germany, and of the largest Bengal tiger, it is more probable that the hyænas found their dead carcases and dragged them to the den, than that they were ever joint tenants of the same cavern. It is however obvious that they were all at the same time inhabitants of antediluvian Yorkshire.

In the case of such minute and burrowing animals as the mouse and weasel, and perhaps the rabbit and fox, it is possible that some of them may have crept into the cave by undiscovered crevices, and there died since the stoppage of its

* For my knowledge of these, and many other bones I have from Kirkdale, I am indebted to a careful examination and comparison of them made by Mr. BROOKS, in his most valuable collection of osteological preparations. Mr. CLIFT also has kindly assisted me at the Royal College of Surgeons in furtherance of the same object.

mouth; and in such case their bones would have been found lying on the surface of the mud before it was disturbed by digging: as no observations were made in season as to this point, it must remain unsettled, till the opening of another cave may give opportunity for more accurate investigation. This uncertainty, however, applies not to any of the extinct species, or to the larger animals, whose habit it is not to burrow in the ground, nor even to those of the smaller ones, *e. g.* the water rat, fragments of whose bones and teeth are found imbedded in the antediluvian stalagmite, and cemented by it both to the exterior and internal cavities of bones belonging to the hyænas and other extinct species, which, beyond all doubt, were lodged in the den before the period of the introduction of the mud. Should it turn out that since this period the cave has been accessible to foxes and weasels, it is possible that some of the birds also may have been introduced by them. The evidence of this, however, rests on a fact not yet carefully ascertained, viz. whether the bones in question were buried, like those of the extinct animals, beneath the mud, or lay on its surface; the state of one of the ravens' bones, containing stalagmite in its central cavity (see Plate XXV. fig. 22, 23), seems to indicate high antiquity; and the quarryman, who was the first to enter the cave, assured me, that he has never seen a single bone of any kind on the surface, nor without digging into the substance of the mud.

As ruminating animals form the ordinary food of beasts of prey, it is not surprising that their remains should occur in such abundance in the cave (see Plate XXII. fig. 1 to 14); but it is not so obvious by what means the bones and teeth of the elephant, rhinoceros, and hippopotamus, were conveyed

thither (see Plate XXI. fig. 1 to 6, and 8 to 10.). On the one hand, the cave is in general of dimensions so contracted (often not exceeding three feet in diameter), that it is impossible that living animals of these species could have found an entrance, or the entire carcases of dead ones been floated into it; moreover, had the bones been washed in, they would probably have been mixed with pebbles and rounded equably by friction, which they are not: on the other hand, it is foreign to the habits of the hyæna to prey on the larger pachydermata, their young perhaps excepted. No other solution of the difficulty presents itself to me, than that the remains in question are those of individuals that died a natural death; for though an hyæna would neither have had strength to kill a living elephant or rhinoceros, or to drag home the entire carcase of a dead one, yet he could carry away, piecemeal, or acting conjointly with others, fragments of the most bulky animals that died in the course of nature, and thus introduce them to the inmost recesses of his den.

Should it be asked why, amidst the remains of so many hundred animals, not a single skeleton of any kind has been found entire, we see an obvious answer, in the power and known habit of hyænas to devour the bones of their prey; and the gnawed fragments on the one hand, and album græcum on the other, afford double evidence of their having largely gratified this natural propensity: the exception of the teeth and numerous small bones of the lower joints and extremities, that remain unbroken, as having been too hard and solid to afford inducement for mastication, is entirely consistent with this solution. And should it be further asked, why we do not find, at least, the entire skeleton of the one or

more hyænas that died last, and left no survivors to devour them; we find a sufficient reply to this question, in the circumstance of the probable destruction of the last individuals by the diluvian waters: on the rise of these, had there been any hyænas in the den, they would have rushed out, and fled for safety to the hills; and if absent, they could by no possibility have returned to it from the higher levels: that they did so perish on the continent is obvious, from the discovery of their bones in the diluvial gravel of Germany, as well as in the caves. The same circumstance will also explain the reason why there are no bones found on the outside of the Kirkdale cave, as described by Busbequius on the outside of the hyænas' dens in Anatolia; for every thing that lay without, on the antediluvian surface, must have been swept far away, and scattered by the violence of the diluvian waters; and there is no reason for believing that hyænas, or any other animals whatever, have occupied the den at any period subsequent to that catastrophe.

Although the evidence to prove the cave to have been inhabited as a den by successive generations of hyænas, appears thus direct, it may be as well to consider what other hypotheses may be suggested, to explain the collection of bones assembled in it.

1st. It may be said, that the various animals had entered the cave spontaneously to die, or had fled into it as a refuge from some general convulsion: but the diameter of the cave, as has been mentioned before, compared with the bulk of the elephant and rhinoceros, renders this solution impossible as to the larger animals; and with respect to the smaller, we can imagine no circumstances that would collect together,

spontaneously, animals of such dissimilar habits as hyænas, tigers, bears, wolves, foxes, horses, oxen, deer, rabbits, water-rats, mice, weasels, and birds.

2d. It may be suggested, that they were drifted in by the waters of a flood: if so, either the carcases floated in entire; or the bones alone were drifted in after separation from the flesh: in the first of these cases, the larger carcases, as we have already stated, could not have entered at all; and of the smaller ones, the cave could not have contained a sufficient number to supply one-twentieth part of the teeth and bones; moreover, the bones would not have been broken to pieces, nor in different stages of decay. And had they been washed in by a succession of floods, we should have had a succession of beds of sediment and stalactite, and the cave would have been filled up by the second or third repetition of such an operation as that which introduced the single stratum of mud, which alone occurs in it. On the other hypothesis, that they were drifted in after separation from the flesh, they would have been mixed with gravel, and at least slightly rolled on their passage; and it would still remain to be shown by what means they were split and broken to pieces, and the disproportion created which exists between the numbers of the teeth and bones. They could not have fallen in through the fissures, for these are closed upwards in the substance of the rock, and do not reach to the surface.

The 3rd, and only remaining hypothesis that occurs to me is, that they were dragged in for food by the hyænas, who caught their prey in the immediate vicinity of their den; and as they could not have dragged it home from any very great distances, it follows, that the animals they fed on all lived

and died not far from the spot where their remains are found.

The accumulation of these bones, then, appears to have been a long process, going on during a succession of years, whilst all the animals in question were natives of this country. The general dispersion of similar bones through the diluvian gravel of high latitudes, over great part of the northern hemisphere, shows that the period in which they inhabited these regions, was that immediately preceding the formation of this gravel, and that they perished by the same waters which produced it. M. Cuvier has moreover ascertained, that the fossil elephant, rhinoceros, hippopotamus, and hyæna, belong to species now unknown; and as there is no evidence that they have at any time, subsequent to the formation of the diluvium, existed in these regions, we may conclude that the period, at which the bones of these extinct species were introduced into the cave at Kirkdale, was antediluvian. Had these species ever re-established themselves in the northern portions of the world since the deluge, it is probable their remains would have been found, like those of the ox, horse, deer, hog, &c. preserved in the post-diluvian accumulations of gravel, sand, silt, mud, and peat, which are referable to causes still in operation, and which, by careful examination of their relations to the adjacent country, can be readily distinguished from those which are of diluvian origin.

The teeth and fragments of bones above described, seem to have lain a long time scattered irregularly over the bottom of the den, and to have been continually accumulating until the introduction of the sediment in which they are now imbedded, and to the protection of which they owe that high state of

preservation they possess. Those that lay long uncovered at the bottom of the den, have undergone a decay proportionate to the time of their exposure; others that have lain only a short time before the introduction of the diluvian mud, have been preserved by it almost from even incipient decomposition.

Thus the phenomena of this cave seem referable to a period in which the world was inhabited by land animals, bearing a general resemblance to those now existing, before the last inundation of the earth; but so completely has the violence of that tremendous convulsion destroyed and remodelled the form of its antediluvian surface, that it is only in caverns that have been protected from its ravages, that we may hope to find undisturbed evidence of events in the period immediately preceding it. The bones already described, and the stalagmite formed before the introduction of the diluvial mud, are what I consider to be the products of the period in question. It was indeed probable, before the discovery of this cave, from the abundance in which the remains of similar species occur in superficial gravel beds, which cannot be referred to any other than a diluvial origin, that such animals were the antediluvian inhabitants of this country; but the proof was imperfect, as it has been said they might have been drifted or floated hither by the waters, from warmer latitudes: but the facts developed in this charnel house of the antediluvian forests of Yorkshire, show that there was a long succession of years in which these animals had been the prey of the hyænas, which like themselves at that time, must have inhabited these regions of the earth; and it is in the diluvial wreck occurring in such latitudes, that similar bones have been

found buried, in the state of grave bones, over great part of northern Europe, as well as North America and Siberia. The catastrophe producing this gravel, appears to have been the last event that has operated generally to modify the surface of the earth, and the few local and partial changes that have succeeded it, such as the formation of deltas, terraces, tufa, torrent-gravel and peat-bogs, all conspire to show, that the period of their commencement was subsequent to that at which the diluvium was formed.*

* It was stated in describing the locality of the cave at Kirkdale, and on comparing it with the fact of its containing the remains of large and small aquatic animals, that there was probably a lake in this part of the country at the period when they inhabited it; and this hypothesis is rendered probable by the form and disposition of the hills that still encircle the Vale of Pickering. (See Map, Plate XV.)

Inclosed on the south, the west, north-west, and north, by the lofty ranges of the Wolds, the Howardian hills, the Hambleton hills, and Eastern Moorlands, the waters of this vale must either run eastward to Filey Bay, or inland towards York; and such is the superior elevation of the strata along the coast, that the sources of the Derwent, rising almost close to the sea, near Scarborough and Filey, are forced to run west and southward fifty miles inland away from the sea, till falling into the Ouse, they finally reach it by turning again eastward through the Humber. The only outlet by which this drainage is accomplished, is the gorge at New Malton; and though it is not possible to ascertain what was the precise extent of this antediluvian lake, or how much of the low districts, now constituting the Vale of Pickering, may have been excavated by the same diluvian waters that produced the gorge; it is obvious, that without the existence of this gorge, much of the district within it would be laid under water; and it is equally obvious, that the gorge is referable to the agency of diluvian denudation, the ravages of which have not, perhaps, left a single portion of the antediluvian surface of the whole earth, which is not torn and re-modelled, so as to have lost all traces of the exact features it bore antecedently to the operations of the deluge.

It is probable, that inland lakes were much more numerous than they are at present, before the excavation of the many gorges by which our modern rivers make their escape; and this is consistent with the frequent occurrence of the remains of the hippopotamus in the diluvian gravel of England, and of various parts of Europe.

It is in the highest degree curious to observe, that four of the genera of animals whose bones are thus widely diffused over the temperate, and even polar regions of the northern hemisphere, should at present exist only in tropical climates, and chiefly south of the equator; and that the only country in which the elephant, rhinoceros, hippopotamus and hyæna are now associated, is Southern Africa. In the immediate neighbourhood of the Cape they all live and die together, as they formerly did in Britain; whilst the hippopotamus is now confined exclusively to Africa, and the elephant, rhinoceros and hyæna are also diffused widely over the continent of Asia.

Such are the principal facts I observed in the interior of the cave at Kirkdale, and such the leading conclusions that seem to arise from them; and I cannot sufficiently lament that I was not present at its first opening, to witness the exact state in which it appeared, before any part of the surface of the mud had been disturbed.

From the description given of the state of the bones, and of the mud and stalactite that accompany them, we may extract the following detailed history of the operations that have successively been going on within the cave.

1st. There appears to have been a period (and if we may form an estimate from the small quantity of stalagmite now found on the actual floor of the cave, a very short one,) during which this aperture in the rock existed, but was not

It is not unlikely that, in this antediluvian period, England was connected with the Continent, and that the excavation of the shallow channel of the Straits of Dover, and of a considerable portion of that part of the German ocean which lies between the east coast of England and the mouths of the Elbe and Rhine, may have been the effect of diluvial denudation. The average depth of all this tract of water is said to be less than thirty fathoms.

tenanted by the hyænas. The removal of the mud, which now entirely covers the floor, would be necessary to ascertain the exact quantity of stalagmite referable to this period; but it cannot be very great, and can only be expected to exist where there is much stalactite also upon the roof and sides.

The 2d period was that during which the cave was inhabited by the hyænas, and the stalactite and stalagmite were still forming. The constant passage of the hyænas in so low a cave, would much interrupt this formation; as they would strike off the former from the roof and sides by their constant ingress and egress; and accordingly in some specimens of the breccia, we find mixt with the bones, fragments of stalactite, that seem to have been thus knocked off from the roof and sides of the cave, whilst it was inhabited by hyænas before the introduction of the mud; I have one example of a hollow stalactitic tube that lay in an horizontal position in the midst of, and parallel to, some long splinters of bone and the unbroken ulna of a rat: all these are united by stalagmite; and it is impossible that this stalactitic pipe could have been formed in any other than a vertical position, hanging from the roof or sides. In other specimens of the breccia, I have split fragments of the teeth of deer and hyæna; and in almost every portion I have seen, either of this breccia or of the antediluvian stalagmite, there are teeth of the water rat. Mr. Gibson possesses a mass exceeding a foot in diameter, composed of fragments of many large bones, mixed with some teeth of rhinoceros and several of the larger animals, and also of rats, all adhering firmly together in a matrix of stalagmite. It did not occur to me, whilst on the spot, to examine whether the bottom of the

cave is any where polished (like the tiger's den before alluded to), in those parts which must have been the constant gangway of the hyænas; but the universal cover of mud by which it is buried, renders it necessary that this should be removed, in order to the observation I suggest. During the formation of this stalactitic matter, no mud appears to have been introduced; and had there been any in the cave at the time whilst the osseous breccia was forming, it would either have excluded all access of the stalagmite to the bones, or have been mixed and entangled with it in very large proportions, forming a spongy mass, such as it does at the root of the stalagmites that lie on its surface.

The 3rd period is that at which the mud was introduced and the animals extirpated, viz. the period of the deluge. I have already stated that the animal remains are found principally in the lower regions of this sediment of mud, which appears to have been introduced in a fluid state, so as to envelope the bony fragments then lying on the bottom of the cave: and the power of water to introduce such sediments is shown by the state of Wokey Hole, and similar caverns in the Mendip Hills, and Derbyshire, which are subject to be filled with water occasionally by heavy land floods. The effect of these floods being to leave on the floor, a sediment of mud precisely similar to that which covers the bones and osseous breccia in the cave of Kirkdale. I have also mentioned that there is no alternation of this mud with beds of bone or of stalagmite, such as would have occurred had it been produced by land floods often repeated; once, and once only it appears to have been introduced; and we may probably consider its vehicle to have been the turbid

waters of the same inundation that produced the diluvial gravel: these would enter and fill the cave, and there becoming quiescent, would deposit the mud suspended in them (as we see daily silt and warp deposited in quiet spots by waters of muddy rivers) along the whole bottom of the den, where it has remained undisturbed ever since. We cannot refer this mud to a land flood, or a succession of land floods, partly for the reasons before stated, and partly from the general dryness of the cave; had it been liable to be filled with muddy water, it would have been so at the time I visited it in December, 1821, at the end of one of the most rainy seasons ever remembered; but even then there were not the slightest symptoms of any such occurrence, and a few scanty droppings from the roof were the only traces of water within the area of the cavern.

The 4th period is that during which the stalagmite was deposited which invests the upper surface of the mud. The quantity of this stalagmite appears to be much greater than that formed in the two periods during, and before which, the cave was tenanted by hyænas. In the whole of this 4th period no creature appears to have entered the cave, with the exception possibly of mice, weasels, rabbits, and foxes, until it was opened last summer, and no other process of any kind appears to have been going on in it except the formation of stalactitic infiltrations; the stratum of diluvial sediment marks the point of time at which the latter state of things began and the former ceased. As there is no mud at all on the top or sides of the cave, we have no mark to distinguish the relative quantities of stalactite formed on these parts during the periods we have been speaking of: should it however contain in any

part a fragment of bone or tooth of any of the extinct animals, it will follow that this part was antediluvial. A farther argument may be drawn from the limited quantity of post-diluvian stalactite, as well as from the undecayed condition of the bones, to show that the time elapsed since the introduction of the diluvian mud, has not been one of excessive length.

The arguments arising from the detail of facts we have been describing, are applicable to the illustration of analogous phenomena, where the evidence of their history is less complete. In our own country there are five other instances of bones similarly deposited in caverns, the origin of some of which, though not before satisfactorily made out, becomes evident as a corollary from the proofs afforded by the cave at Kirkdale: these are in Glamorganshire, Somersetshire, Derbyshire, and Devonshire.

1. The first is in the parish of Nicholaston, on the coast of Glamorganshire, at a spot called Crawley Rocks, in Oxwich Bay, about twelve miles S.W. of Swansea; it was discovered in the year 1792, in a quarry of lime-stone, on the property of T. M. TALBOT, Esq. of Penrice Castle, and no account of it has, I believe, been ever published; some of the bones however are preserved in the collection of Miss TALBOT, at Penrice; they are as follows:

Elephant——Three portions of large molar teeth.
Rhinoceros—Right and left ossa humeri.
One atlas bone.
Two molar teeth of upper jaw.
Ox————First phalangal bone of left fore foot.
Stag————Lower extremity of the horn.

Stag———Three molar teeth.
One first phalangal bone, right leg.
Hyæna———Two canine teeth, much worn.

These bones were found in a cavity of mountain lime-stone, which was accidentally intersected, like the cave at Kirkdale, in working a quarry: they have a slight ochreous incrustation, and a little earthy matter adhering to them; but are not in the least degree rolled; and the condyles of the two humeri of the rhinoceros, belonging to different individuals, have in each case been entirely broken off, as if by gnawing. The two canine teeth of hyæna (worn down to the stumps), that were found in the same cave with them, afford ground for probable conjecture as to the means by which those bones were thus broken, as well as introduced to this cave in Glamorganshire.*

2. The next case I shall mention is that of teeth and bones of elephants and other animals discovered in the Mendip Hills in cavities of mountain lime-stone, which were lined, and nearly filled with ochreous clay. These are preserved in the collection of the Rev. Mr. Catcott, in the City Library at Bristol. The following account of them is extracted by my friend the Rev. W. D. Conybeare, from Mr. Catcott's MS. notes; he has added also a few explanatory observations.

"The ochre pits were worked about the middle of the last century, near the summit of the Mendip Hills, on the S. of

* On comparing one of these humeri of the rhinoceros with a similar bone from the cave at Kirkdale, I found in each case both extremities of the bone broken or gnawed off exactly to the same point, i. e. just so far as was sufficient to extract the marrow and take off the most spongy portions of the extremities, whilst the parts remaining were only the hardest and most compact cylindrical portions of the centre of the bones in question.

the village of Hutton, near Banwell, at an elevation of from three hundred to four hundred feet above the level of the sea: they are now abandoned.

"The ochre was pursued through fissures in the mountain lime-stone, occasionally expanding into larger cavernous chambers, their range being in a steep descent, and almost perpendicular. Thus, in opening the pits, the workmen, after removing eighteen inches of vegetable mould, and four feet of rubbly ochre, came to a fissure in the lime-stone rock, about eighteen inches broad, and four feet long. This was filled with good ochre, but as yet no bones were discovered; it continued to the depth of eight yards, and then opened into a cavern about twenty feet square, and four high; the floor of this cave consisted of good ochre, strewed on the surface of which were multitudes of white bones, which were also found dispersed through the interior of the ochreous mass. In the centre of this chamber, a large stalactite depended from the roof; and beneath, a similar mass rose from the floor, almost touching it: in one of the side walls was an opening about three feet square, which conducted through a passage eighteen yards in length, to a second cavern, ten yards in length, and five in breadth, both the passage and cavern being filled with ochre and bones; another passage, about six feet square, branched off laterally from this chamber about four yards below its entrance; this continued nearly on the same level for eighteen yards; it was filled with rubbly ochre, fragments of lime-stone rounded by attrition, and lead ore confusedly mixed together; many large bones occurring in the mass; among which four magnificent teeth of an elephant (the whole number belonging to a single skull) were found; another shaft was sunk from the surface perpendicularly into

his branch, and appears to have followed the course of a fissure, since it is said that all the way nothing appeared but rubble, large stones, ochre, and bones: in the second chamber, immediately beyond the entrance of the branch just described, there appeared a large deep opening, tending perpendicularly downwards, filled with the same congeries of rubble, ochre, bones, &c.; this was cleared to the depth of five yards; this point, being the deepest part of the workings, was estimated at about thirty-six yards beneath the surface of the hill; a few yards to the west of this another similar hole occurred, in which was found a large head, which we shall have occasion presently to notice."

The bones from this cavern, preserved in Mr. CATCOTT's cabinet in the Bristol library, are the teeth and fragments of some bones of the elephant; and similar remains of horses, oxen, and two species of stag, besides the skeleton, nearly complete, of a fox. There are also molar teeth of the hog, and a large tusk of the upper jaw; (see Pl. XXV. fig. 30, 31, 32, 33.) This tusk probably belonged to the head mentioned in his MS. as having been found in the pit above described, and of which the following particulars are specified:—"The head was stated by the workmen to have been about three or four feet long, fourteen inches broad at the top, or head part, and three inches at the snout. It had all the teeth perfect, and four tusks, the larger tusks about four inches long out of the head, and the lesser about three inches."* The tusk now

* The head here described, is evidently that of a hog; the account of its length being exaggerated by the workmen, from whose report alone Mr. CATCOTT gives the measures of it. The head itself was lost or destroyed before he had seen it.

preserved is about three inches long, its enamel is fine, it is longitudinally striated, and on one side of the apex truncated and worn flat by use.

On the summit of Sandford Hill, on the east of Hutton, bones of the elephant were also, according to Mr. CATCOTT's MSS., discovered four fathoms deep among loose rubble. Some farther detail of the bones found in the cave at Hutton are given as a note in Mr. CATCOTT's Treatise on the Deluge (page 361, 1st. edition), in which he specifies six molar teeth of the elephant, one of them lying in the jaw, part of a tusk, part of a head, four thigh bones, three ribs, with a multitude of lesser bones, belonging probably to the same animal. "Besides these (he adds), we picked up part of a large deer's horn very flat, and the slough of a horn (or the spongy porous substance that occupies the inside of the horns of oxen), of an extraordinary size, together with a great variety of teeth and small bones belonging to different species of land animals. The bones and teeth were extremely well preserved, all retaining their native whiteness, and, as they projected from the sides and top of the cavity, exhibited an appearance not unlike the inside of a charnel-house."

It appears to me most probable, from the description given of these bones and horns, that they were not all dragged in by beasts of prey, but some of them, at least, drifted in by water, and the presence of pebbles seems to add credibility to this conjecture.

3. Another case of fossil fragments of bone has been discovered by Mr. MILLER, of Bristol, in a cavity of mountain lime-stone, near Clifton, by the turnpike gate on Derdham

Down: these are not rolled, but have evidently been fractured by violence: they are partially incrusted with stalactitic matter, and the broken surfaces have also an external coating of thin ochreous stalactite, showing the fracture to have been ancient; one specimen, the property of Mr. MILLER, displays the curious circumstance of a fossil joint of the horse; it is the tarsus joint, in which the astragalus retains its natural position between the tibia and os calcis; these are held together by a stalactitic cement, and were probably left in this position by some beast of prey that had gnawed off the deficient portions of the tibia and os calcis.

4. A fourth case is that of some bones and molar teeth of the elephant, found in another cavity of mountain limestone at Balleye, near Wirksworth, in Derbyshire, in the year 1663; one of these teeth is now in the collection of Mr. WHITE WATSON, of Bakewell. There is, I believe, no detailed account of the circumstances under which these remains were found, farther than that the cavity was intersected in working a lead mine; they might possibly have been introduced in the same manner as those at Kirkdale and Crawley Rocks.

5. The fifth and last example which I am acquainted with, is that described by Sir EVERARD HOME and J. WHIDBY, Esq. in the Philosophical Transactions for 1817, as discovered at Oreston, near Plymouth, by Mr. WHIDBY, in removing the entire mass of a hill of transition lime-stone for the construction of the Breakwater. This lime-stone is full of caverns and fissures, such as may be seen at Stonehouse and elsewhere along the edge of the cliffs; that in which the bones were found was fifteen feet wide, twelve high, and

forty-five long, and about four feet above high water mark; it was filled with solid clay (probably diluvian mud) in which the teeth and bones were imbedded, and was intersected in blasting away the body of the rock to make the Breakwater. The state of the teeth and bones was precisely the same with that of those found at Crawley rocks, they were much broken, but not in the slightest degree rounded by attrition, and Sir Everard Home has ascertained them to belong exclusively to a species of rhinoceros. A similar discovery of teeth and bones was made in 1820, in a smaller cavern, distant one hundred and twenty yards from the former, being one foot high, eighteen wide and twenty long, and eight feet above the high water mark; a description of its contents is given in the Philosophical Transactions for 1821, by the same Gentlemen. It contained no stalactite, which abounds in many of the adjacent caverns. Sir Everard Home describes these teeth and bones as belonging to the rhinoceros, deer, and a species of bear.

Mr. Whidby is of opinion, that neither of these caverns had the appearance of ever having had any opening to the surface, or communication with it whatever; an opinion in which I can by no means acquiesce; though I think it probable that the openings had, as at Kirkdale, been long ago filled up with rubbish, mud, stalactite, or fragments of rock re-united, as sometimes happens, into a breccia as solid as the original rock, and overgrown with grass. It is now too late to appeal to the evidence of facts, as the rock in which the cave existed is entirely removed; but the circumstances of similar caverns that have communication with the surface, either open or concealed, both in this neighbourhood, and in compact lime-stone rocks of all ages and formations, and in all

countries, added to the identity of species and undecayed state of the animal remains which they contain, render the argument from analogy perfect, to show that the bones at Oreston are not coeval, and have only an accidental connection with the rock in the cavities of which they were found.

It by no means follows, from the certainty of the bones having been dragged in by beasts of prey to the small cavern at Kirkdale, that those of similar animals must have been introduced in all other cases in the same manner; for, as these animals were the antediluvian inhabitants of the countries in which the caves occur, it is possible, that some may have retired into them to die, others have fallen into the fissures by accident and there perished, and others have been washed in by the diluvial waters. By some one or more of these three latter hypotheses, we may explain those cases in which the bones are few in number and unbroken, the caverns large and the fissures extending upwards to the surface; but where they bear marks of having been lacerated by beasts of prey, and where the cavern is small, and the number of bones and teeth so great, and so disproportionate to each other as in the cave at Kirkdale, the only adequate explanation is, that they were collected by the agency of wild beasts. We shall show hereafter, that in the case of the German caves, where the quantity of bones is greater than could have been supplied by ten times the number of carcases which the caves, if crammed to the full, could ever have contained, they were the bones of bears that lived and died in them during successive generations.

We may now proceed to consider how far the circum-

stances of the caves we have been examining in England, appear consistent with those of analogous caverns in other parts of the world. The history of the diluvian gravel of the Continent, and of the animal remains contained in it, appears altogether identical with that of our own; and with respect to the bones that occur in caverns, the chief difference seems to be, that on the Continent some of the caves have their mouths open, and have been inhabited in the *post-diluvial* period by animals of now existing species. Thus at Gailenreuth the great extinct bear (Ursus spelæus) occurs, together with the Yorkshire species of extinct hyæna, in a cave, the mouth of which has no appearance of having ever been closed, and which at this moment would, probably, have been tenanted by wild beasts, had not the progress of human population extirpated them from that part of Germany.

For a description of the cavern at Gailenreuth, (which I visited in 1816) of which, in Plate XXVI. I have given a sectional representation, I must refer to the work of ROSENMULLER, published at Weimar in 1804, in folio, with engravings of nearly all the bones composing the skeleton of the extinct bear, the size of which approached nearly to that of a horse; and for a description of the caves at Blankenburg, to an account by ESPER and LEIBNITZ, published at Brunswick.

M. ROSENMULLER says, he has never seen the remains of the elephant and rhinoceros in the same cavern with those of bears; and that he has found the bones of wolves, foxes, horses, mules, oxen, sheep, stags, roebucks, badgers, dogs, and men;* and that the number of all these is in no propor-

* M. ESPER has found in one of the caverns containing bears' bones, fragments of urns, which from their form were probably made at least 800 years ago.

tion to that of the bears. The bones of all kinds occur in scattered fragments. One entire skeleton only of the Ursus spelæus is said to have been found by BRUCKMANN, in a cave in the Carpathians, and to have been sent to Dresden. He adds, that the different state of these bones shows that they were introduced at different periods, and that those of all the animals last enumerated, including man, are in much higher preservation than those of the bears and hyænas.

Thus it appears that the bones which are in most perfect preservation, and belong to existing species, have been introduced during the *post-diluvian* period; whilst the extinct bears and hyæna are referable to the antediluvian state of the earth. In corroboration of this, I found in 1820, in the collection of the Monastery of Kremsminster, near Steyer, in Upper Austria, skulls and bones of the Ursus spelæus in consolidated beds of diluvial gravel, forming a pudding-stone, and dug for building near the monastery; from which it appears that this species of bear lived in the period immediately preceding the formation of that diluvium; and the same thing has been already shown of the extinct hyæna in the gravel of France and Germany.

M. ROSENMULLER states that in all the caverns he has examined, the bones are disposed nearly after the same manner; sometimes scattered separately, and sometimes accumulated in beds and heaps of many feet in thickness; they are found every where from the entrance to the deepest and most secret recesses; never in entire skeletons, but single bones mixed confusedly from all parts of the body, and animals of all ages. The skulls are generally in the lowest part of the beds of bone, having from their form and weight sunk

or rolled downwards, as the longer and lighter bones were moved and disturbed continually by the living animals passing over them; the lower jaws are rarely found in contact with, or near to the upper ones, as would follow from the fact last mentioned.* They are often buried in a brown argillaceous or marly earth, as in the cases of Gailenreuth, Zahnloch, and in the Hartz, which earth, from an analysis by M. FRISCHMAN, seems to contain a large proportion of animal matter derived from the decay of the fleshy parts of the bears.

In the caves of Gailenreuth and Mockas, a large proportion of the bones is invested with stalactite. Even entire beds, and heaps of them many feet thick, are sometimes cemented together by it, so as to form a compact breccia. Occasionally they adhere by stalactite to the sides of the cavern, but are never found in the substance of the rock itself. At Sharzfelden, and in the Carpathians, they have been found enveloped with agaric mineral (lac lunæ); they have undergone no alteration of form, but the larger bones are generally separated from their epiphyses. Their usual colour is yellowish white, but brown where they have lain in dark coloured earth, as at Lichtenstein. At Mockas their degree of decay is by far the greatest. Even the enamel of the teeth is far gone, and the bones are perfectly white, having lost all their animal

* At Kirkdale, not one skull, and few, if any, of the larger bones are found entire; for these had all been broken up by the hyænas to extract the brains and marrow; and in their strong and worn out teeth we see the instruments by which they were thus destroyed. The bears, on the other hand, not being exclusively carnivorous, nor having teeth fitted for the cracking of large bones, have left untouched the osseous remains of their own species.

gluten, and acquired the softness and spongy appearance, as well as colour, of calcined bones; still their form is perfect, and substance inflexible, and when struck, they ring like metallic bodies falling to the ground. These retain simply their phosphate of lime. In other caverns they are usually less decayed, but they sometimes exfoliate and crack on exposure to air, and the teeth, particularly, are apt to split and fall to pieces, as are also those at Kirkdale.*

M. Rosenmuller is decidedly of opinion with M. Cuvier, that the bears' bones are the remains of animals which lived and died through successive generations in the caves in which we find them; nay, even that they were also born in the same caves. In proof of which he has found some bones of a bear, that must have died immediately after birth, and other bones of individuals that must have died young. This is analogous to the case of numerous teeth of young hyænas with fangs not formed; and the jaws of two that had not shed their first teeth, which I found at Kirkdale.

Most of the arguments which I have used to show that the bones in Yorkshire cannot have been accumulated by the action of one, or of a succession of floods, apply with equal force to the cave at Gailenreuth, and it is unnecessary to repeat them.

The above description of the cave at Gailenreuth, extracted from Rosenmuller, and confirmed by my own observations

* It is a curious fact, that of the numerous caves in the calcareous hills near Muggendorf, that flank the valley of the Weisent-stream, those on the north chain contain not a fragment of the bones of the Ursus spelæus, while those on the south side are full of them. This may probably be explained by supposing the mouths of the former to have been closed in the antediluvian period, and afterwards laid open by denudation.

on the spot, may be taken as an example of the state of the other caves on the Continent, of which it is superfluous here to say any thing farther, than to subjoin a list given by M. CUVIER of the most important of them, and to refer to the fourth Volume of his *Animaux fossiles*, for farther details taken from the authors by whom these caves have been described.

The caves alluded to are as follows:

1. That of Bauman, in the county of Blankenberg, in Brunswick, on the east border of the Hartz forest, and described by LEIBNITZ.

2. That of Sharzfels, in Hanover, in the south border of the Hartz, described by LEIBNITZ, DELUC, and BRUCKMANN.

BEHRENS, in his Hercynia Curiosa, speaks of several more in the neighbourhood of the Hartz; from most of these the bones were collected during a long course of years, and sold for their imaginary medicinal virtues under the name of Licorne.

3. The caves that next attracted attention were those of the Carpathians, and the bones found in them were at first known by the name of dragons' bones, and have been described by HAYNE and BRUCKMANN.

4. But the most richly furnished are the caves of Franconia, described by ESPER and ROSENMULLER, near the sources of the Mayn, in the vicinity of Bamberg and Bayreuth, at the villages of Gailenreuth, Mockas, Rabenstein, Kirch-a-horn, Zahnloch, Zewig, and Hohen Mirchfeld.

5. A fifth locality occurs at Glücksbrun, near Meinungen, on the south border of the Thuringerwald.

6. And a sixth in Westphalia, at Kluterhoehle and Sund-

wich, in the country of Mark. M. CUVIER states, that the bones found in these caverns are identical over an extent of more than 200 leagues; that three-fourths of the whole belong to two species of bear, both extinct; the Ursus spelæus and Ursus arctoideus, and two-thirds of the remainder to extinct hyænas. A very few to a species of the cat family, being neither a lion, tiger, panther, or leopard, but most resembling the jaguar, or spotted panther of South America. There is also a wolf or dog (not distinguishable from a recent species), a fox and polecat. He adds that, in the caves thus occupied, there occur no remains of the elephant, rhinoceros, horse, ox, tapir, or any of the ruminantia or rodentia. In this respect they differ materially from that of Yorkshire; but such variation is consistent with the different habits of bears and hyænas, arising from the different structure of their teeth and general organization; from which it follows, that bears prefer vegetable food to that of animals, and, when driven to the latter, prefer sucking the blood to eating the flesh, whilst hyænas are beyond all other beasts addicted to gnawing bones.

From this circumstance it is rendered probable, that in the caves inhabited chiefly by bears, the bones of other animals should be extremely rare. But unless there be an error in the statement of M. DELUC (Lettres, vol. iv. p. 588), that a tooth found in the cave at Scharzfels was ascertained by M. HOLLMAN to be that of a rhinoceros; and of ESPER, that large cervical vertebræ of an elephant were found by M. FRISCHMAN in the cave of Schneiderloch; it follows, that these two animals occur, though very rarely, in the caves of Germany, and they may have been introduced by the few hyænas that

occasionally inhabited them; that they lived in the neighbourhood of these caves, in the period immediately preceding the formation of the diluvium, is probable, from the occurrence in it of the bones of the elephant and rhinoceros near the caves of Scharzfels and Alterstein, mentioned by BLUMENBACH. (Archaeologia Telluris, p. 15.)

The fact mentioned by M. CUVIER, of the same hyæna being common to the caves and gravel of France and Germany, and that ascertained by myself, of the Ursus spelæus occurring in the gravel of Upper Austria, proves both these extinct species to have been the antediluvian contemporaries of the extinct elephant and rhinoceros; there is therefore no anachronism in finding the remains of the two latter in a den that was occasionally inhabited by such hyænas and bears.

With respect to the analogies of the diluvian sediment and the stalactite in Germany and Yorkshire, in the case of the open caves that have been disturbed and ransacked for centuries, it is hopeless to expect evidence of what was the precise state of these deposits in each individual cavern at the time it was first entered. Still there is information respecting some that have been recently discovered, which is to our purpose. It is stated, that a sediment of this kind was found on the sides and floor of the cave at Glucksbrun, near Meinungen, when it was newly opened in cutting a road in 1799, and that in all the other caverns also there is mud, but no rounded pebbles. M. DELUC, in describing the matrix in which the bones are lodged in the cave at Scharzfels, says, "le fait est donc simplement, que le sol de ces cavernes est d'une terre calcaire," "qu'en creusant cette couche molle, on en tire quantité de fragmens d'os; et qu'il s'y trouve aussi des concrétions pier-

reuses qui renferment des os." DELUC, Lettres, vol. iv. p. 590. These concretions with bones appear analogous to the stalagmitic concretions at Kirkdale, and the soft calcareous earth by which they are covered, resembles its stratum of mud. Again, the resemblance holds also in the existence both of bones and soft mud in the smallest recesses of the caverns. He says, p. 589, " Il faut en quelques endroits se trainer sur le ventre, par dessous la pierre dure pour continuer a y creuser." This is an exact description of the state of the extremities of the cave at Kirkdale at the present moment.

LEIBNITZ, in his description of this same cavern, has the following words to the same purpose, " Limo nigricante vel fusco infectum est solum." LEIBNITZ, Protogaea, p. 65.

ESPER thus describes the state of the floor near the entrance of one of the largest caverns at Gailenreuth. " Dans toute la contrée le terrain est marneux, mêlé avec du limon, et tire sur le jaune, mais ici on trouve une terre moins limoneuse dans une profondeur considérable. Je ne prétends pas encore la prendre absolument pour une terre animale telle qu'est sans contredit la terre qui se trouve plus bas, mais probablement elle doit y être rapportée, p. 9. This again is consistent with the circumstances of the cave at Kirkdale, the mud, thus dubiously spoken of, being probably of diluvial origin, and reposing on, and being mixed with, the animal earth that had been formed before its introduction. The absence of black animal earth at Kirkdale, results from the fact of the flesh, and great part even of the bones of the animals introduced to it, having been eaten by the hyænas.

The identity of time and circumstances which I am endeavouring to establish between the German and English

caverns, does not, however, depend so much on comparisons between the stalactitic matter and earthy sediments which they contain, as on the agreement in species of the animals entombed in them, viz. in the agreement of the animals of the English caves, with those of the diluvian gravel of the greater part of Europe; and, in the case of the German caves, on the identity of the extinct bear with that of the diluvian gravel of Upper Austria, and the extinct hyæna with that of the gravel at Canstadt, in the valley of the Necker; and at Eichstadt, in Bavaria; to these may be added the extinct rhinoceros, elephant, and hippopotamus, which are common to gravel beds as well as caves. And hence it follows, that the period at which all these caverns were inhabited by the animals in question, was antecedent to the formation of that deposit of gravel, which it seems to me impossible to ascribe to any other origin than a transient deluge, affecting universally, simultaneously, and at no very distant period, the entire surface of our planet.

The bones found in these caverns are considered by M. CUVIER, to be of older date than those of the osseous breccia, which, at Gibraltar and various places along the coast of the Mediterranean and Adriatic, occur in vertical fissures of limestone. This breccia contains fragments of bones and teeth of various ruminating and gnawing animals; that is, of ox, deer, antelope, sheep, rabbits, rats, mice; also of the horse and ass, of snakes and birds, mixed with land shells, and angular fragments of the adjacent rock; all united into a solid breccia by ochreous stalactite. The greater number of these animals agree with species that now exist, and are supposed by M. CUVIER to have fallen into the fissures in

the period succeeding the last retreat of the waters. I do not see why some of them may not also have fallen in during that earlier period in which the bears occupied the caves of Germany, and the hyænas that in Yorkshire; for some of the animals found at Kirkdale seem to agree in species with those that occur in the fissures; but as they are at the same time not distinguishable from existing species, the argument arising from this resemblance is imperfect. The discovery of the extinct elephant, rhinoceros, hippopotamus, bear, and hyæna in this breccia, should it ever be made, would be decisive of the question.

For an account of the bones accumulated in these fissures, I must again refer to the works of M. Cuvier, which contain more sound and clear philosophical reasoning on the early state of habitation on our planet, and a more valuable collection of authentic facts relating to the history of its fossil animals of the higher orders, than can be found in all the books that have ever yet been written on the subject.

APPENDIX.

It was mentioned, when speaking of Gailenreuth, that human remains had been discovered there in the same cave with the bones of antediluvian animals, but that they are of comparatively low antiquity.

Three analogous cases have been noticed in this country in cavities of mountain lime-stone, at Burringdon, in Somersetshire, and in Glamorganshire and Caermarthenshire; and these also are attended by circumstances which indicate them to be of post-diluvian origin.

1. The discovery of human bones incrusted with stalactite, in a cave of mountain lime-stone at Burringdon, in the

Mendip-hills, is explained, by this cave having either been used as a place of sepulture in early times, or been resorted to for refuge by wretches that perished in it, when the country was suffering under one of the numerous military operations which, in different periods of our early history, have been conducted in that quarter. The mouth of this cave was nearly closed by stalactite, and many of the bones were incrusted with it. In the instance of a skull, it had covered the inside as well as the outside of the bone; and I have a fragment from the inside, which bears in relief casts of the channel of the veins along the interior of the skull. The state of these bones affords indications of very high antiquity; but there is no reason for not considering them post-diluvian. Mr. Skinner, on examination of this cave, found the bones disposed chiefly in a recess on one side, as in a sepulchral catacomb; and in the same neighbourhood, at Wellow, there is a large artificial catacomb of high antiquity, covered by a barrow, and constructed after the manner of that at New Grange, near Slane, in the county of Meath, of stones successively overlapping each other till they meet in the roof. In this were found the remains of many human bodies. A description of it may be seen in the Archæologia for 1820.

2. Mr. Dillwyn has observed two analogous cases in the mountain lime-stone of South Wales; one of these was discovered, in 1805, near Swansea, in a quarry of lime-stone at the Mumbles, where the workmen cut across a wedge-shaped fissure, diminishing downwards, and filled with loose rubbish, composed of fragments of the adjacent lime-stone, mixed with mould. In this loose breccia lay, confusedly, a large number of human bones, that appear to be the remains of bodies thrown in after a battle, with no indications of regular

burial; they were about 30 feet below the present upper surface of the lime-stone rock.

3. The other case occurred, in 1810, at Llandebie, in Caermarthenshire, where a square cave was suddenly broken into, in working a quarry of solid mountain lime-stone on the north border of the great coal basin. In this cave lay about a dozen human skeletons in two rows at right angles to each other. The passage leading to this cave had been entirely closed up with stones for the purpose of concealment, and its mouth was completely grown over with grass.

It is obvious, that in neither of these cases, are the bones referable to so high an era as those of the wild beasts that occur in the caves at Kirkdale, and elsewhere.

P.S. As this paper was going to the press, I have been gratified to hear that my conjecture, as to the abundance of such caverns as that at Kirkdale, has been verified by the discovery of another cave (containing chambers lined with stalactite, and having on its bottom mud, and bones imbedded in the mud), in a quarry close to the town of Kirby Moorside, on the property of C. Duncombe, Esq., who has judiciously taken every precaution to secure it from injury, till some qualified person shall be present to observe, and record the undisturbed appearance presented by its interior. Should it be in my power, as I hope it may, to assist at its farther opening, I shall communicate the result to the Royal Society.

It is recollected also, that about 20 years ago, another cavity, containing bones, was discovered on the north of Kirby Moorside, but none of them have been preserved.

Though it is probable, as I have stated, that such caverns are not uncommon, we shall cease to wonder that they are

so rarely brought to light, when we consider the number of accidental circumstances that must concur to lead to such an event. 1st, The existence of caverns is an accidental circumstance in the interior of the rock, of which the external surface affords no indication, when the mouth is filled with rubbish and overgrown with grass. 2d, The presence of bones is another accidental circumstance, though probably not an uncommon one in the case of those caves, the mouths of which were accessible to the wild beasts that inhabited this country in the period immediately preceding the deluge. 3d, A farther requisite is, the intersection of one of these caves in which there happen to be bones, by a third accident, viz. the working of a stone quarry by workmen who have sufficient curiosity or intelligence to notice and speak of what they find, and this to persons who may be willing or able to appreciate, and give publicity to the discovery. The necessary concurrence of all these contingencies renders it probable, that however great may be the number of subterraneous caverns, in an inland country, very few of them will ever be discovered, or if discovered, be duly appreciated. Those I have mentioned in Devon, Somerset, Derby and Glamorganshire, were all laid open by the accidental operations of a quarry or mine.

May 24, 1822. I have this day received the entire lower jaw of an hyæna from Lawford, near Rugby, in Warwickshire. It was found by Andrew Bloxam, Esq. in the same diluvial clay and gravel with the bones of elephant and rhinoceros. This is the first instance of the remains of hyæna being noticed in the diluvium of England. The animal must have perished by the same catastrophe which extirpated the hyænas, and closed the den at Kirkdale, and which swept together the remains of elephant, rhinoceros and hyæna in the diluvian gravel of the Continent. The support which this recent discovery gives to my arguments on the cave in Yorkshire, is too obvious to require pointing out.

EXPLANATION OF THE PLATES.

All the Drawings are of the natural size, unless where it is expressed to the contrary.

Plate XV.

Map of the country adjacent to the cave at Kirkdale, showing the entire drainage of the vale of Pickering to be effected through the gorge at Malton, the stoppage of which would at once convert it into an inland lake.

Plate XVI.

Fig. 1. View of the mouth of the cave at Kirkdale, in the face of a quarry, near the brow of a low hill.

Fig. 2. Section of the cave before the mud had been disturbed.

A. Stratum of mud covering the floor of the cave to the depth of one foot, and concealing the bones.

B. Stalagmite incrusting some of the bones, and formed before the mud was introduced.

C. C. Stalagmite formed since the introduction of the mud, and spreading horizontally over its surface.

D. Insulated stalagmite on the surface of the mud.

E. E. Stalactites hanging from the roof above the stalagmites.

Fig. 3. Ground plan of the cave, by W. Salmond, Esq. showing its extent, ramifications, and the fissures by which is is intersected.

PLATE XVII.

1. Portion of the left upper jaw of the modern hyæna from the Cape.

2. Inside view of No. 1.

3. Analogous portion of the left upper jaw of the fossil hyæna from Kirkdale.

4. Inside view of No. 3, with the tooth of a water-rat adhering by stalagmite to a broken portion of the palate.

5. Fragment from Kirkdale, showing five incisor teeth of the upper jaw, much worn down, and the inside of the palate.

PLATE XVIII.

1. Outside view of the right lower jaw of the modern Cape hyæna.

2. Analogous portion of lower jaw of the Kirkdale hyæna, being nearly one-third larger.

3. Inside view of No. 2.

PLATE XIX.

1. Fragment of the right lower jaw of an hyæna, showing the convex surface of the jaw and its teeth, that lay uppermost in the den, to be deeply worn by friction, and to have received a polish. The enamel, and one-third of the substance of the teeth and bone on this side have been worn away.

2. Concave surface of No. 1, having no marks of friction, polish, or decay: the enamel on this side of the teeth is perfect and unchanged.

3. Fragment of the right lower jaw of a young hyæna, having the convex surface only polished as in No. 1; and showing the cavities in which the second set of teeth were rising to succeed the first set; one of which, the posterior

molar tooth, still remains in its place, having its enamel on this side worn away, as in the teeth of No. 1.

4. Inside or concave surface of No. 3, has suffered no friction or polish, and the enamel of the tooth is perfect and fresh as in No. 2.

5. Metatarsal bone of hyæna.

6, 7. Phalanges of the toe of an animal not ascertained.

8. Claw bone of the toe of an hyæna.

9, 10, 11. Metacarpal bone and two phalanges of the toe of hyæna.

12. Claw bone of the toe of an hyæna.

Plate XX.

1. Canine tooth or tusk of a bear (Ursus spelæus).

2. Inside view of posterior molar tooth of the lower jaw on the left side of hyæna.

3. Outside view to No. 2.

4. Largest canine tooth or tusk of hyæna found at Kirkdale.

5. Tusk of an animal of the tiger kind.

6. Outside view of right posterior molar tooth of the lower jaw of a tiger.

7. Inside view of No. 6. On comparing 6 and 7 with 2 and 3, it will be observed that in 6 and 7, the angle near the middle part of the crown is less obtuse than in 2 and 3, and that the two lobes which project at the base of the crown of 2 and 3 are wanting in 6 and 7.

8. Tusk of fox.

9. Incisor tooth of fox.

10. Inside view of No. 9.

11. Small molar tooth of fox.

12. Great molar tooth of the right lower jaw of fox; outside view.

13. Inside view of No. 12.

14. Penultima of upper jaw, right side, of fox.

15. }
16. } Molar teeth of wolf.
17. } Inside view.
18. } Outside view of No. 17.

19. Tooth of an animal not ascertained.

20, 21, 22, 23, 24, 25, 26, 27. Outside and inside views of four molar teeth of an animal not ascertained: they are all extremely thin, and have deep furrows worn on them.

28, 29. Posterior tooth and penultima of a weasel, left upper jaw (twice the natural size).

30, 31. Two views of the same tooth of an animal not ascertained, perhaps a seal.

PLATE XXI.

1. Small molar tooth of a very young elephant, being the average size of those found in the den.

2. Fragment of a still younger elephant's tooth.

3. Molar tooth of upper jaw of rhinoceros.

4. Inside view of molar tooth of lower jaw of rhinoceros.

5. Crown of No. 4, as seen from above.

6. Outside view of No. 4.

7. Molar tooth of the upper jaw of a horse.

8. } Two views of a molar tooth of hippopotamus not yet
9. } worn down.

10. Molar tooth of hippopotamus, having the summits of the crown worn down.

PLATE XXII.

1. Posterior molar tooth of the lower jaw of an ox.

2 Crown of No. 1.

3. Posterior molar tooth of the right lower jaw of a species of deer.

4. Molar tooth of the upper jaw of an ox.

5. Molar tooth of the lower jaw of a calf.

6. Side view of No. 5.

7. Molar tooth of the upper jaw of an ox.

8. Outside view of No. 7.

9. Molar tooth of the upper jaw of a very large species of deer, equalling in size the largest elk, but differing in form.

10. Outside view of No. 9.

11. Molar tooth of the upper jaw of a second species of deer, equalling in size the largest red deer.

12. Outside view of No. 11.

13. } Inside and outside views of a rising molar tooth of a
14. } third species of deer, of the size of a large fallow deer.

Plate XXIII.

1. Outside view of a molar tooth of the lower jaw of a large species of deer.

2. Inside view of No. 1.

3. Base of the horn of a large deer, measuring nine inches and three quarters in circumference, which corresponds exactly in size with that of a very large English red deer in the Anatomy School at Oxford.

4. Base of a horn similar to No. 1, having two antlers near its lower extremity, and measuring seven inches and three quarters in circumference.

5. Base of a deer's horn, having the lowest antler at the distance of three inches and a half from the lower extremity, and measuring eight inches in circumference.

PLATE XXIV.

1. Coronary bone of a horse.

2. First phalangal bone of a very large ox; side view.

3. Under side of No. 2.

4, 5. Astragalus of a large ox: two different sides of the same bone.

6. Album græcum, showing a small sphere adhering to the larger one, and an indentation of the sides of both by pressure from a third sphere.

7. Astragalus of hyæna.

8. Side view of No. 7.

9. Astragalus of fox.

10. Side view of No. 9.

11. Astragalus of water-rat.

12. Os calcis of water-rat.

13. Os calcis of fox.

14. Os calcis of rabbit.

15, 16. Internal metatarsal bone of rabbit, having lost the epiphysis.

17, 18. Metatarsal bone of rabbit, retaining the epiphysis at the lower extremity.

PLATE XXV.

1. Lower jaw of a water-rat.

2. Lower incisor tooth of No. 1.

3. Upper incisor of water-rat.

4. Anterior molar tooth of lower jaw of water-rat.

5. No. 4, magnified.

6. Crown of No. 4, magnified.

7. Jaw of a mouse.

8 and 9. Teeth of No. 7, magnified four times.

10. Anterior molar tooth of the upper jaw of a rabbit.

11. Os innominatum of a young water-rat.

12. Tibia of a water-rat.

13. Lower epiphysis of femur of water-rat, twice magnified.

14. Femur of water-rat, twice magnified.

15. Ulna of water-rat.

16. Tail vertebra of water-rat.

17. Anterior extremity of No. 16.

18. Posterior extremity of No. 16.

19. Right ulna of a raven; anterior extremity.

20. Outside view of No. 19, showing the points of attachment of the quill feathers.

21. Right ulna of a raven, showing the other extremity of No. 19.

22, 23. Other views of No. 20, showing the cavity to be nearly filled with stalagmite.

24. Right ulna of a lark, showing the attachments of the quill feathers.

25. Inside view of No. 24.

26. Left ulna of a very large species of pigeon.

27. Inside view of No. 26.

28. Right coracoid process of the scapula of a small species of duck or widgeon.

29. Inside view of No. 28.

30. Tusk of the upper jaw of a large hog, polished obliquely near its apex, and having a molar tooth of hog adhering to it, near its base, by an ochreous crust.

31. View of the opposite side of No. 30.

32. Large molar tooth of hog in a fragment of the lower jaw, slightly incrusted with ochre.

33. Small molar tooth of hog.

Plate XXVI.

Vertical section of the great cave at Gailenreuth, near Bamberg, in Franconia; drawn from a sketch made on the spot by Professor Buckland, A. D. 1816.

A. Entrance passage, varying from five to eight feet in height, terminated externally by an open mouth in the steep side of a hill, and internally expanding into the cavern B.: a few bones are scattered irregularly along the floor.

B. First large chamber, having stalactites of all sizes hanging from its roof, and numerous bones of bears scattered on its floor.

C. Second chamber, separated from B by a perpendicular precipice, but having, probably, other lateral communications with it. On its floor the bones lie scattered more abundantly than in B.

D. Large aperture, descending obliquely downwards from C, and having cart loads of loose bones in it.

E. Mass composed chiefly of bones cemented by stalagmite, and forming a compact breccia.

F. Very long and low passage, connecting the chamber C with the vertical fissure G. The length of this passage is not expressed, for want of room; it can be traversed only by crawling on the hands and knees.

G. Vertical fissure of considerable depth, and about three feet in breadth; the only mode of descent is by supporting the hands and feet on niches cut in the opposite sides of the fissure.

H. Oven-shaped cavity, which has been produced artificially by extracting bones and skulls from the osseous breccia.

XVII. *Communication of a curious appearance lately observed upon the Moon. By the Rev.* Fearon Fallows. *In a Letter addressed to* John Barrow, *Esq. F. R. S.*

Read February 28, 1822.

Cape Town, Cape of Good Hope,
December 13, 1821.

Dear Sir,

I take the earliest opportunity of communicating to you a curious appearance which I lately observed upon the moon. My present means of making observations of this kind are indeed very limited, as the large telescopes, destined for the Cape Observatory, have not yet arrived. Still, however, it is right to have phenomena of this kind recorded, though the description may, from the want of proper instruments, be imperfect.

About eight o'clock in the evening of the 28th of November last, the sky being extremely clear, and the moon shining with a brilliancy which I never observed in England, my attention was drawn to a whitish spot on the dark part of the moon's limb, sufficiently luminous to be seen with the naked eye. Lest I might be mistaken, I requested Mr. Fayror, the assistant astronomer, to look at the moon attentively, and inform me whether he could observe any bright appearance upon the dark part of it. We both agreed in the identity of the spot, and remarked that now and then it seemed to flash with considerable lustre. Mr. Fayror having in his possession a good achromatic telescope, which Mr. Troughton had

given him previous to our departure from England, I requested the loan of it for a few nights, so that I might be able to examine this appearance more minutely. Having directed the telescope to the moon, I immediately recognised the luminous spot, which seemed like a star of the sixth magnitude, and *three others* much smaller, but *one* of these more brilliant than the one we had seen with the naked eye. The largest spot was surrounded by a nebulous appearance. I could not perceive any thing of the kind about the small brilliant spot. The two others were similar to faint nebulæ, increasing in intensity towards the middle, but without any defined luminous point. As I am not yet in possession of a micrometer, by means of which the situation of these spots might be ascertained, you must rest satisfied with this imperfect description. On the evening of the 29th, the sky being equally favourable for observation as on the former one, I found that the large spot was, at the least, as bright as before, two others were nearly invisible, and the small brilliant spot had disappeared. I was unable to make any farther observations, as a strong south-east wind began to blow with great violence on the 30th, accompanied with rain, and which lasted several days. I wait with great anxiety for the next new moon, when, if the sky be clear, I shall not fail to examine it as carefully as my means at present will permit.

The telescope which I used is 4 feet long, and at the time of observation its magnifying power was 100.

I remain, Dear Sir,

very truly yours,

FEARON FALLOWS.

To John Barrow, Esq.

XVIII. *On the difference in the appearance of the teeth and the shape of the skull in different species of Seals.* By Sir Everard Home, *Bart. V. P. R. S.*

Read February 28, 1822.

At a time when geology is pursued with so much ardour, I am induced to lay before the Society the following facts respecting the skull and teeth of the seal, as their being known will be an advantage when incrusted or fossil remains of that animal are met with.

The accompanying drawings were made 30 years ago, at the time Mr. Hunter was preparing to lay before the Society some observations on the skulls of wolves and bears found in the caves of the principality of Bayreuth, in Germany. Among these, so great was the difference in the form of the skull of the young bear and the old, that where the jaws had been destroyed, it required the eye of an anatomist to determine that the skull really belonged to the bear.

While engaged upon that subject, I was surprised to find in the seal tribe, not only that the skull in the large species from the South Seas, differed exceedingly in its appearance from that of the smaller ones in the Northern Ocean, but that the teeth were equally unlike. At that time the facts had less importance; the study of geology being little attended to, the subject escaped my memory, till the recent strides made by Cuvier, Buckland, and others, have induced me to resume it.

EXPLANATION OF THE PLATES.

Plate XXVII.

Represents the skull of the large seal so many years deposited in the British Museum, from the South Seas.

Plate XXVIII.

Shows the skull of a seal shot near the Orkney Isles, by a gentleman who went two years in succession for that purpose, and afterwards gave the skeleton to Mr. Hunter. This animal had been known for 30 summers to come to the same rock, and lie basking in the sun; it had a grey beard.

Plate XXIX.

Is taken from the skull of a seal in the Museum of the Royal College of Surgeons in Lincoln's Inn Fields, presented by Mr. Chevalier: this proves to have been brought by Mr. Kearn, in a Whaler, from New Georgia, near the ice towards the South Pole. The circumstance of most importance in this communication is, that in all the three skulls the teeth differ in form: this may arise from the different kinds of food on which the animals lived.

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METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1821.

1821	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
Jan. 1	8 0	26	38	29,83	25	Rain this Month 1,854 Inches.	N	1	Cloudy and hazy.
	2 0	29	44	29,82	30		ENE	1	Cloudy and hazy.
2	8 0	25	39	29,72	25		E	1	Cloudy.
	2 0	27	43	29,61	31		E	1	Cloudy.
3	8 0	28	40	29,36	25		E	1	Cloudy.
	2 0	32	41	29,31	33		E	1,2	Cloudy. [the night.
4	8 0	27	38	29,35	27		N	1	Cloudy, a gale of wind in
	2 0	30	43	29,36	32		NW	1	Cloudy.
5	8 0	30	40	29,28	30		E	1	Cloudy and hazy.
	2 0	32	43	29,21	33		E	1	Snow.
6	8 0	36	41	29,10	30		SE	1	Cloudy and foggy.
	2 0	40	46	29,17	42		E	1	Thick fog.
7	8 0	35	41	29,19	35		N	1	Cloudy and foggy.
	2 0	40	42	29,21	36		NE	1	Cloudy and hazy.
8	8 0	37	41	29,16	35		E	1	Cloudy.
	2 0	40	48	29,11	43		E	1	Cloudy and hazy.
9	8 0	38	45	29,00	37		—	1	Thick fog.
	2 0	41	49	28,98	42		SW	1	Fog.
10	8 0	39	46	29,08	39		S	1	Cloudy.
	2 0	44	47	29,11	45		WSW	1	Cloudy and hazy.
11	8 0	41	48	29,06	39		E	1	Rain.
	2 0	42	48	29,12	45		SE	1	Rain.
12	8 0	45	50	29,23	39		S	1,2	Cloudy.
	2 0	48	54	29,25	50		SSE	1	Cloudy and hazy.
13	8 0	46	50	29,49	45		W	1	Fine.
	2 0	49	54	29,51	50		SSW	1	Cloudy.
14	8 0	44	51	29,22	44		N	1	Rain.
	2 0	43	49	29,61	51		S	1	Cloudy.
15	8 0	36	47	30,08	36		E	1	Hazy.
	2 0	41	51	29,93	43		ESE	1	Cloudy.
16	8 0	44	51	29,68	36		W	1	Fine.
	2 0	47	55	29,87	48		WNW	1	Cloudy.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Rain	Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	39	52	30,13	38	Rain this Month 1,854 Inches.	W	1	Hazy.
	2	0	47	53	30,14	49		WSW	1	Cloudy and hazy.
18	8	0	46	51	30,21	39		W	1	Cloudy.
	2	0	51	57	30,23	52		WSW	1	Cloudy and hazy.
19	8	0	47	54	30,31	46		S	1	Cloudy.
	2	0	48	57	30,28	51		WSW	1	Cloudy and hazy.
20	8	0	44	53	30,30	42		W	1	Hazy.
	2	0	50	57	30,41	52		WSW	1	Hazy and cloudy.
21	8	0	36	52	29,61	36		—	1	Thick fog.
	2	0	44	53	30,61	51		—	1	Fine.
22	8	0	39	49	30,64	36		W	1	Cloudy.
	2	0	43	53	30,60	46		W	1	Cloudy.
23	8	0	40	52	30,73	39		E	1	Cloudy.
	2	0	42	57	30,76	45		E	1	Cloudy.
24	8	0	33	47	30,72	33		E	1	Fine.
	2	0	37	54	30,67	42		W	1	Hazy.
25	8	0	35	49	30,60	30		SW	1	Cloudy.
	2	0	44	54	30,60	45		E	1	Foggy.
26	8	0	39	52	30,58	35		N by E	1	Cloudy and hazy.
	2	0	41	55	30,54	45		N	1	Cloudy.
27	8	0	36	51	30,42	36		SE	1	Cloudy.
	2	0	38	55	30,35	42		ESE	1	Cloudy.
28	8	0	34	48	30,22	34		SE	1	Cloudy.
	2	0	38	47	30,21	40		S by E	1	Cloudy.
29	8	0	32	44	30,21	32		E	1	Fine.
	2	0	41	55	30,18	42		S by E	1	Fine.
30	8	0	43	47	30,24	39		SW	1	Cloudy.
	2	0	47	58	30,27	48		W	1	Fine.
31	8	0	44	51	30,35	43		SW	1	Cloudy and hazy.
	2	0	50	55	30,37	51		SW	1	Cloudy.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Feb. 1	8	0	46	53	30,38	45	Rain this Month 0,278 Inches.	SW	1	Cloudy.
	2	0	50	57	30,35	52		WSW	1,2	Cloudy.
2	8	0	47	54	30,17	45		W	1	Cloudy.
	2	0	59	59	30,28	61		NW	1	Fine.
3	8	0	37	52	30,33	36		W	1	Fine.
	2	0	47	56	30,23	49		S	1,2	Cloudy.
4	8	0	43	51	29,98	41		S	1	Cloudy.
	2	0	45	51	29,93	47		SWbyS	1	Cloudy.
5	8	0	33	48	30,59	33		N	1	Hazy.
	2	0	42	56	30,71	54		NW	1	Fine.
6	8	0	35	48	30,77	32		W by S	1	Hazy.
	2	0	42	56	30,77	44		W by S	1	Fine.
7	8	0	34	49	30,70	34		SW	1	Fine.
	2	0	43	54	30,68	42		S	1,2	Fine.
8	8	0	37	50	30,62	36		SSW	1	Fine.
	2	0	46	58	30,54	47		SSE	1	Fine.
9	8	0	36	51	30,23	35		SE	1	Hazy.
	2	0	45	59	30,08	46		NNE	1	Fine.
10	8	0	40	51	30,24	36		NE	1,2	Cloudy.
	2	0	45	55	30,31	45		E	1	Fine.
11	8	0	36	49	30,35	36		E	1	Hazy and cloudy.
	2	0	40	51	30,28	44		E	1	Fine.
12	8	0	34	47	30,23	33		N	1	Hazy.
	2	0	40	54	30,23	41		NNE	1	Fine.
13	8	0	35	48	30,29	35		ESE	1	Cloudy.
	2	0	39	51	30,28	40		N	1	Pleasant but cloudy.
14	8	0	34	47	30,23	34		N	1	Cloudy and dark.
	2	0	37	53	30,22	39		N	1	Cloudy.
15	8	0	34	50	30,35	34		W	1	Thick fog.
	2	0	36	53	30,38	38		E	1	Cloudy and hazy.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Feb. 16	8	0	33	48	30,51	32	Rain this Month 0,278 Inches.	NE	1	Cloudy.
	2	0	38	51	30,51	39		N	1	Cloudy.
17	8	0	32	48	30,40	32		S	1	Cloudy and hazy.
	2	0	36	50	30,29	38		S	1	Cloudy and hazy.
18	8	0	32	45	30,17	30		NNW	1	Cloudy.
	2	0	37	45	30,12	38		W	1	Cloudy and hazy.
19	8	0	31	43	30,24	30		N	1	Fine.
	2	0	38	47	30,31	40		W	1	Cloudy but pleasant.
20	8	0	28	42	30,27	28		WbyN	1	Hazy.
	2	0	38	47	30,13	40		WSW	1	Cloudy.
21	8	0	36	45	30,15	29		NW	1	Hazy.
	2	0	43	50	30,16	44		N	1	Fine.
22	8	0	36	47	30,27	35		E	1	Cloudy and hazy.
	2	0	40	53	30,28	44		W	1	Cloudy.
23	8	0	30	48	30,30	30		W	1	Cloudy.
	2	0	40	57	30,24	41		W by S	1	Fine.
24	8	0	31	49	30,15	31		W	1	Hazy.
	2	0	40	52	30,11	42		SSW	1	Cloudy but pleasant.
25	8	0	35	47	30,07	31		NE	1	Cloudy and hazy.
	2	0	43	47	30,04	45		WSW	1	Cloudy and hazy.
26	8	0	34	47	30,05	34		E	1	Cloudy.
	2	0	34	47	30,07	43		E	1	Cloudy and hazy.
27	8	0	25	43	29,88	25		E	1	Fine.
	2	0	35	52	29,77	36		ESE	1	Fine.
28	8	0	32	47	29,30	26		E	1	Cloudy; snow in the night.
	2	0	33	49	29,21	35		NNE	1	Snow and sleet.

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1821	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Mar. 1	7	0	37	48	29,33	37	Rain this Month 1,917 Inches.	E	1	Cloudy.
	2	0	46	52	29,38	47		ESE	1	Cloudy.
2	7	0	40	54	29,80	38		SW	1	Fine.
	2	0	47	60	29,34	49		SE	1	Rain.
3	7	0	45	54	29,74	48		WSW	1	Cloudy.
	2	0	51	59	29,72	52		W	1	Rain.
4	7	0	48	54	29,61	45		W	1	Cloudy.
	2	0	54	56	29,61	55		W	1	Fine.
5	7	0	39	52	29,84	39		E	1	Cloudy.
	2	0	36	56	29,94	54		NE	1	Cloudy.
6	7	0	34	50	29,83	31		E	1	Cloudy.
	2	0	40	55	29,62	40		E	1	Rain.
7	7	0	41	51	29,52	39		W	1	Cloudy.
	2	0	50	63	29,58	51		SW	1	Cloudy.
8	7	0	45	55	29,21	45		W	1	Cloudy.
	2	0	51	51	29,23	54		WNW	1	Cloudy.
9	7	0	43	57	29,58	43		W by N	1	Fine.
	2	0	54	60	29,51	55		SW	1	Rain.
10	7	0	48	59	29,57	48		SW	1	Fine.
	2	0	57	66	29,65	59		SSW	1	Fair.
11	7	0	44	57	29,79	43		W	1	Fine.
	2	0	52	57	29,82	58		W	1	Fine.
12	7	0	42	55	29,92	41		W	1	Hazy.
	2	0	51	63	29,95	54		W	1	Showery.
13	7	0	42	55	30,00	41		W	1	Hazy.
	2	0	52	66	30,02	54		W	1	Fine.
14	7	0	44	58	30,07	42		NW	1	Fine.
	2	0	54	65	30,16	49		NNE	1	Fine and clear.
15	7	0	37	54	30,36	37		SW	1	Fine.
	2	0	48	66	30,38	53		SW	1	Fine.
16	7	0	41	57	30,30	37		E	1	Hazy.
	2	0	52	65	30,24	52		S	1	Fine.

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for March, 1821.

1821	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Mar. 17	7	0	39	56	30,06	38	Rain this Month 1,917 Inches.	W	1	Fog.
	2	0	50	64	29,93	52		SW	1	Fine.
18	7	0	42	54	29,48	39		W	1	Fine.
	2	0	48	56	29,45	52		WNW	1	Cloudy and showery.
19	7	0	39	53	29,20	38		W	1	Fine.
	2	0	46	56	29,18	48		W by N	2	Cloudy.
20	7	0	40	51	29,30	37		W	1	Fine.
	2	0	47	60	29,27	49		NW	1	Cloudy, but pleasant.
21	7	0	40	54	29,23	38		W	1	Cloudy.
	2	0	45	60	29,33	49		N	1	Fine.
22	7	0	39	53	29,53	37		E	1	Fine.
	2	0	45	59	29,75	46		N by E	1	Fine, rather showery.
23	7	0	36	53	30,05	34		NNE	1	Fine.
	2	0	47	59	30,07	47		NW	1	Cloudy.
24	7	0	40	52	29,84	36		S	2	Cloudy.
	2	0	48	56	29,68	50		S	1	Cloudy. [wind in the night.
25	7	0	42	51	29,35	42		S by E	1	Rain: a violent storm of
	2	0	50	58	29,38	52		W	1	Cloudy.
26	7	0	39	51	29,61	36		W	1	Fine.
	2	0	48	58	29,56	51		S	1	Cloudy.
27	7	0	41	51	29,22	40		SSW	1	Cloudy.
	2	0	47	56	29,33	50		NNW	1	Cloudy.
28	7	0	40	51	29,16	39		S	2	Rain.
	2	0	47	56	29,06	48		SSE	1	Cloudy.
29	7	0	42	52	29,06	42		S	1	Rain.
	2	0	47	59	29,21	48		SW	1	Cloudy.
30	7	0	38	53	29,63	37		W	1	Fine.
	2	0	48	69	29,71	50		SW	1	Fine.
31	7	0	41	51	29,41	39		S	2	Cloudy, rain in the night.
	2	0	48	56	29,38	51		NNW	1	Cloudy.

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for April, 1821.

1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
April 1	7	0	37	51	29,58	36	Rain this Month 1,137 Inches.	W	1	Fine.
	2	0	51	55	29,61	51		W	1	Fine.
2	7	0	48	53	29,27	42		SSW	1.2	Rain.
	2	0	55	58	29,25	59		W	1	Cloudy.
3	7	0	42	53	29,31	40		W	2	Fair.
	2	0	50	58	29,29	55		NW	1	Fine.
4	7	0	41	54	29,31	39		W	1	Hazy.
	2	0	53	61	29,32	53		WNW	1	Fine.
5	7	0	41	54	29,45	40		N	1	Fine.
	2	0	47	60	29,66	53		N	1	Fine.
6	7	0	39	55	30,03	37		NW	1	Fine.
	2	0	58	59	30,07	59		WNW	1	Cloudy.
7	7	0	46	56	29,97	39		W	1	Rain.
	2	0	59	60	30,06	59		NNW	1	Cloudy.
8	7	0	50	57	30,12	49		W	1	Cloudy.
	2	0	63	61	30,12	64		NW	1	Fine.
9	7	0	48	58	30,02	47		W	1	Cloudy.
	2	0	60	62	29,93	63		NNW	1	Fine.
10	7	0	51	59	29,81	50		W	1	Rather hazy.
	2	0	61	62	29,73	62		W	1	Cloudy.
11	7	0	50	59	29,66	48		S	1	Cloudy.
	2	0	60	63	29,58	62		S	2	Cloudy.
12	7	0	44	57	29,36	42		W	1	Fine.
	2	0	53	62	29,31	59		SW	2	Cloudy.
13	7	0	45	57	29,32	41		W	1	Fine.
	2	0	53	61	29,45	54		W	2	Cloudy.
14	7	0	45	55	29,67	40		S	1	Fine.
	2	0	61	57	29,32	62		S	2	Cloudy.
15	7	0	42	53	29,52	39		SSW	1	Cloudy.
	2	0	60	55	29,50	62		SSW	1	Showery.
16	7	0	43	52	29,47	38		S	1	Fine.
	2	0	61	57	29,40	63		S	1	Fine.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Apl. 17	7	0	40	53	29,42	38		W	1	Fine, rather hazy.
	2	0	59	58	29,64	59		W	1	
18	7	0	45	53	29,64	41		W	1	Hazy.
	2	0	55	58	29,69	56		NW	1	Cloudy.
19	7	0	49	54	29,70	44		S	2	Cloudy.
	2	0	53	55	29,56	55		S	2	Rain.
20	7	0	50	54	29,51	49	Rain this Month 1,137 Inches.	SW	1	Cloudy; rained hard in [the night.
	2	0	60	56	29,55	60		ESE	1	Cloudy dull weather, thunder and lightning at 8 P. M.
21	7	0	53	56	29,73	50		NE	1	Cloudy.
	2	0	58	58	29,82	60		N	1	Cloudy.
22	7	0	48	56	29,97	45		N	1	Fine.
	2	0	57	58	29,87	59		SE	1	Fine.
23	7	0	52	57	29,56	49		E	1	Hazy.
	2	0	66	62	29,49	68		E	1	Cloudy.
24	7	0	56	58	29,40	53		S	1	Fine.
	2	0	64	68	29,50	66		S	1	Fine.
25	7	0	60	60	29,63	54		SE	1	Fine.
	2	0	68	69	29,57	70		S	1	Fine.
26	7	0	60	63	29,75	58		NNE	1	Hazy.
	2	0	71	68	29,68	72		E	1	Fine.
27	7	0	58	64	29,66	56		SW	1	Cloudy.
	2	0	64	64	29,71	72		SW	1	Cloudy, but pleasant.
28	7	0	59	62	29,81	52		WSW	1	Fine.
	2	0	66	65	29,81	67		S	1	Fine.
29	7	0	53	62	29,81	56		S	1	Fine.
	2	0	61	67	29,84	67		S	1	Fine.
30	7	0	53	61	30,01	51		N	1	Cloudy, rather hazy.
	2	0	53	61	30,04	55		N	1,2	Cloudy.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Rain	Winds. Points.	Winds. Str.	Weather.
May 1	7	0	49	59	30,04	48	Rain this Month 1,093 Inches.	N	1	Cloudy and hazy.
	2	0	56	61	29,97	57		ESE	1	Fine.
2	7	0	53	59	29,87	48		N b E	1	Cloudy.
	2	0	62	65	29,84	64		E	1	Cloudy.
3	7	0	52	59	29,75	51		S	1	Cloudy.
	2	0	65	62	29,69	66		S	1	Cloudy.
4	7	0	55	61	29,66	53		W	1	Fine.
	2	0	66	64	29,68	70		S	1	Fine.
5	7	0	57	61	29,62	52		SW	1	Cloudy.
	2	0	63	65	29,53	68		S	1	Fine.
6	7	0	52	60	29,43	47		W	1	Cloudy.
	2	0	59	62	29,49	64		WSW	1	Fine.
7	7	0	49	59	29,80	45		W	1	Fine.
	2	0	59	62	29,86	62		W	1	Fine.
8	7	0	53	59	29,88	51		SW	2	Rain.
	2	0	61	61	29,95	63		SW	1	Cloudy.
9	7	0	47	55	30,14	43		W	1	Fine.
	2	0	61	64	30,15	62		NNW	1	Fine.
10	7	0	50	58	30,25	47		WNW	1	Fine.
	2	0	60	59	30,21	62		W	1	Cloudy.
11	7	0	51	57	30,01	47		W	1	Cloudy.
	2	0	66	60	29,96	66		W by N	1	Cloudy.
12	7	0	56	59	29,86	54		NW	1	Cloudy.
	2	0	64	60	29,80	66		NW	1	Cloudy.
13	7	0	48	56	29,38	44		W	2	Cloudy.
	2	0	54	58	29,31	56		WSW	1	Cloudy, rain at intervals.
14	7	0	46	54	29,30	42		N	1	Fine.
	2	0	54	62	29,35	55		NW	1	Fine.
15	7	0	46	56	29,17	44		SSW	1	Cloudy.
	2	0	52	58	29,17	54		WSW	1	Fine.
16	7	0	46	55	29,69	41		W	1	Fine.
	2	0	56	61	29,81	56		W	1	Fine.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
May 17	7	0	47	55	29,95	43	Rain this Month 1,093 Inches.	S	1	Cloudy.
	2	0	52	56	29,91	56		SE	1	Rain.
18	7	0	50	56	29,87	48		N	1	Fine.
	2	0	60	61	30,03	61		W	1	Cloudy.
19	7	0	47	56	30,20	45		N	1	Fine.
	2	0	60	60	30,22	60		S	1	Fine.
20	7	0	49	56	30,17	46		N	1	Cloudy.
	2	0	55	57	30,18	61		E	1	Cloudy.
21	7	0	47	55	30,14	42		SE	1	Fine.
	2	0	51	57	30,05	52		E	1	Cloudy.
22	7	0	47	54	29,99	41		N	1	Cloudy.
	2	0	62	56	29,93	65		ESE	1	Fine.
23	7	0	46	54	29,78	43		N	1	Cloudy.
	2	0	50	55	29,76	52		E	1	Rain.
24	7	0	42	52	29,92	39		N	1	Cloudy.
	2	0	58	55	29,98	63		W	1	Fine.
25	7	0	45	52	29,94	40		W	1	Cloudy.
	2	0	60	55	29,74	58		W	1	Fine.
26	7	0	42	53	29,73	38		NW	1	Fine.
	2	0	48	58	29,77	57		N	1	Fine.
27	7	0	44	53	29,86	37		N	1	Cloudy.
	2	0	60	57	29,91	65		N	1	Cloudy, but pleasant.
28	7	0	47	53	29,88	43		W	1	Cloudy.
	2	0	54	56	29,95	56		W	1	Fine.
29	7	0	46	52	30,08	42		W	1	Fine.
	2	0	57	56	30,11	59		N	1	Fine.
30	7	0	49	56	30,18	45		W	1	Fine.
	2	0	57	57	30,18	62		SE	1	Fine.
31	7	0	45	55	30,16	42		E	1	Cloudy.
	2	0	57	63	30,11	59		E	1	Fine.

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1821	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
June 1	7	0	50	58	30,04	55	Rain this Month 1,833 Inches.	E	1	Fine.
	2	0	68	62	30,03	68		E	1	Cloudy.
2	7	0	57	59	30,00	53		N	1	Fine.
	2	0	68	62	29,96	69		E	1	Fine.
3	7	0	53	60	29,91	57		N	1	Cloudy.
	2	0	66	66	29,84	70		E	1	Fine.
4	7	0	56	60	29,70	54		NE	1	Rain.
	2	0	63	63	29,63	67		E	1	Fine.
5	7	0	58	62	29,69	54		W	1	Cloudy.
	2	0	67	66	29,72	68		W	1	Fine.
6	7	0	57	62	29,82	54		W	1	Fine.
	2	0	66	66	29,90	69		N	1	Cloudy.
7	7	0	55	59	29,81	51		W	1	Cloudy.
	2	0	62	61	29,66	67		W	1	Fine; rain at 4 P.M. [night.
8	7	0	55	61	29,57	52		N	1	Cloudy; much rain in the
	2	0	58	60	29,71	63		NE	1	Cloudy.
9	7	0	49	58	29,80	45		WNW	1	Cloudy.
	2	0	56	57	29,78	59		WSW	1	Cloudy.
10	7	0	47	56	29,72	43		W by N	1	Cloudy.
	2	0	52	54	29,72	55		S	1	Fine.
11	7	0	51	55	29,83	43		NNE	1	Cloudy.
	2	0	53	56	29,94	55		N	1	Cloudy.
12	7	0	50	55	30,16	46		N	1	Fine.
	2	0	54	57	30,23	55		N	1	Cloudy.
13	7	0	54	56	30,26	47		N	1	Fine.
	2	0	57	57	30,23	58		N	1	Cloudy.
14	7	0	52	55	30,28	44		NE	1	Cloudy.
	2	0	60	59	30,29	62		W	1	Fine.
15	7	0	52	57	30,30	49		N	1	Hazy.
	2	0	65	60	30,28	66		W	1	Fine.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
June 16	7	0	55	57	30,21	52	Rain this Month 1,833 Inches.	W	1	Cloudy.
	2	0	61	57	30,21	65		SW	1	Cloudy.
17	7	0	53	58	30,28	51		N	1	Cloudy.
	2	0	58	57	30,28	61		NW	1	Cloudy.
18	7	0	53	57	30,32	50		NE	1	Cloudy and hazy.
	2	0	64	65	30,30	65		N	1	Fine.
19	7	0	54	57	30,24	48		NE	1	Fine.
	2	0	62	65	30,18	65		NW	1	Fine.
20	7	0	53	59	30,10	49		N	1	Cloudy.
	2	0	59	62	30,06	63		NNE	1	Fine.
21	7	0	53	57	30,06	48		NNE	1	Fine.
	2	0	59	59	30,07	60		E	1	Cloudy.
22	7	0	51	57	30,15	46		N	1	Fine.
	2	0	57	58	30,16	60		S	1	Fine.
23	7	0	53	52	30,17	51		NE	1	Cloudy.
	2	0	58	58	30,17	59		S	1	Cloudy.
24	7	0	53	57	30,13	48		N	1	Cloudy.
	2	0	58	57	30,08	59		E	1	Cloudy.
25	7	0	55	57	30,10	51		N by E	1	Cloudy.
	2	0	65	61	30,11	65		E	1	Fine.
26	7	0	56	58	30,12	52		N	1	Cloudy.
	2	0	64	61	30,08	65		WSW	1	Fine.
27	7	0	55	57	30,06	52		N	1	Fine.
	2	0	65	66	30,07	66		N	1	Fine.
28	7	0	58	59	30,15	55		N by W	1	Cloudy.
	2	0	65	64	30,16	66		SSW	1	Fine.
29	7	0	55	58	30,14	51		NE	1	Hazy.
	2	0	69	66	30,08	70				Fine.
30	7	0	60	61	29,90	55		SW	1	Fine.
	2	0	72	67	29,88	73		S	1	Fine.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
July 1	7	0	61	64	29,58	59	Rain this Month 2,236 Inches.	W	1	Cloudy.
	2	0	58	62	29,61	74		E	1	Rain.
2	7	0	54	60	29,75	51		E	1	Rain.
	2	0	66	61	29,78	68		NNE	1	Rain.
3	7	0	51	59	29,77	51		E	1	Cloudy.
	2	0	58	59	29,82	60		E	1	Fine.
4	7	0	52	57	30,01	46		N	1	Fine.
	2	0	60	61	30,09	61		N	1	Fine, rather cloudy.
5	7	0	56	59	30,15	52		N W	1	Fine.
	2	0	64	61	30,15	66		W	1	Fine.
6	7	0	56	60	30,02	54		W	1	Hazy.
	2	0	63	61	29,89	65		NW	1	Cloudy.
7	7	0	55	58	29,78	52		NNW	1	Cloudy and hazy.
	2	0	58	60	29,85	61		SSE	1	Fine.
8	7	0	53	57	29,98	48		N	1	Cloudy.
	2	0	59	57	30,04	60		SW	1	Fine.
9	7	0	56	58	30,08	51		N N W	1	Fine, rather hazy.
	2	0	65	63	30,07	67		N W	1	Fine.
10	7	0	59	60	30,09	56		N	1	Cloudy and hazy.
	2	0	66	62	30,09	67		WNW	1	Fine.
11	7	0	55	60	30,13	53		W	1	Hazy.
	2	0	65	61	30,11	67		W	1	Cloudy.
12	7	0	55	58	30,07	50		E	1	Fine.
	2	0	63	62	30,05	65		E	1	Fine.
13	7	0	56	60	30,00	51		SW	1	Fine.
	2	0	65	62	29,92	66		S by E	1	Fine.
14	7	0	53	60	29,82	55		W	1	Cloudy.
	2	0	66	64	29,78	67		W	1	Fine.
15	7	0	56	60	29,71	53		W	1	Rain.
	2	0	57	61	29,68	68		E	1	Rain.
16	7	0	58	60	29,94	53		N	1	Fine.
	2	0	57	64	30,02	58		N by W	1	Cloudy.

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1820	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
July 17	7 0	58	61	30,20	55	Rain this Month 2,236 Inches.	SSW	1	Cloudy.
	2 0	67	64	30,26	68		W	1	Cloudy.
18	7 0	58	61	30,30	55		E	1	Fine.
	2 0	69	68	30,28	71		E	1	Fine.
19	7 0	60	64	30,11	57		E	1	Cloudy.
	2 0	72	71	29,99	73		E	1	Fine.
20	7 0	63	65	29,82	59		W	1	Fine.
	2 0	71	68	29,84	73		WSW	1,2	Cloudy.
21	7 0	58	64	29,76	62		W	1	Fine.
	2 0	69	69	29,77	71		W	1	Fine.
22	7 0	58	64	29,58	56		S	1,2	Rain.
	2 0	69	70	29,56	70		NW	1	Fine.
23	7 0	59	62	29,58	55		S	1	Fine.
	2 0	64	65	29,61	69		WNW	1	Fine.
24	7 0	59	63	29,71	55		W	1	Cloudy; rain in the night.
	2 0	67	66	29,74	68		SW	1	Fine.
25	7 0	59	63	29,56	56		S	1	Rain.
	2 0	68	66	29,76	70		S	1	Fine.
26	7 0	59	63	29,88	55		W	1,2	Cloudy.
	2 0	66	66	29,91	69		W	1	Cloudy.
27	7 0	59	61	29,92	55		W	1	Fine.
	2 0	67	66	29,94	68		NW	1	Fine.
28	7 0	57	63	29,96	54		W	1	Hazy.
	2 0	65	62	29,91	68		W	1	Fine.
29	7 0	56	62	30,02	52		W	1	Fine.
	2 0	67	63	30,01	68		WNW	1	Fine.
30	7 0	55	61	29,93	55		SSE	1	Rain.
	2 0	65	64	29,88	68		W	1	Rain.
31	7 0	63	64	29,90	56		W	1	Fine.
	2 0	71	66	29,91	72		W	1	Cloudy.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Aug. 1	7	0	61	64	29,93	60	Rain this Month 2,097 Inches.	SW	1	Cloudy.
	2	0	70	66	29,89	72		W	1	Cloudy.
2	7	0	61	63	30,07	57		N	1	Fine.
	2	0	71	67	30,09	72		W	1	Fine.
3	7	0	62	64	30,07	59		N	1	Cloudy.
	2	0	71	66	30,08	73		N	1	Fine.
4	7	0	62	64	30,06	60		S	1	Fine.
	2	0	73	74	30,07	74		E	1	Fine.
5	7	0	65	66	29,97	61		E	1	Fine.
	2	0	76	72	29,96	77		E	1	Fine.
6	7	0	67	68	29,87	63		W	1	Fine.
	2	0	76	69	29,87	77		W	1	Fine.
7	7	0	61	67	29,96	56		W	1	Fine.
	2	0	69	69	29,97	73		NW	1	Cloudy.
8	7	0	60	65	29,90	55		W	1	Cloudy.
	2	0	64	63	29,64	69		W	1	Rain.
9	7	0	60	64	29,49	57		W	1	Fine.
	2	0	67	67	29,50	69		N	1	Fine.
10	7	0	57	61	29,48	54		W	1	Cloudy.
	2	0	66	65	29,47	68		W	1,2	Fine, rather cloudy.
11	7	0	58	61	29,55	54		W	1	Cloudy.
	2	0	67	64	29,64	68		WNW	1	Cloudy.
12	7	0	56	61	29,85	50		W	1	Fine.
	2	0	67	65	29,89	68		W	1	Fine.
13	7	0	57	61	29,95	53		W	1	Cloudy.
	2	0	68	65	29,95	70		WSW	1	Cloudy.
14	7	0	58	62	29,80	58		W	1	Rain.
	2	0	67	64	29,66	70		NNW	1	Cloudy.
15	7	0	58	62	29.90	53		N	1	Fine.
	2	0	68	68	29,97	70		NW	1	Fine.
16	7	0	61	64	30,03	58		W	1	Cloudy.
	2	0	71	69	30,04	72		W	1	Cloudy.

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1821	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Rain	Winds. Points.	Str.	Weather.
Aug. 17	7	0	64	65	30,10	62	Rain this Month 2,097 Inches.	W	1	Cloudy.
	2	0	69	71	30,18	70		W	1	Fine.
18	7	0	63	67	30,00	62		W	1	Cloudy dull weather.
	2	0	67	67	30,15	70		W	1	Cloudy.
19	7	0	58	64	30,16	54		N	1	Fine.
	2	0	70	72	30,17	71		WSW	1	Fine.
20	7	0	63	66	30,18	58		W	1	Cloudy and hazy.
	2	0	74	74	30,19	75		E	1	Fair.
21	7	0	64	67	30,17	60		E	1	Fine, rather hazy.
	2	0	75	78	30,15	76		E	1	Fine.
22	7	0	65	67	30,14	63		E	1	Hazy thick weather.
	2	0	75	76	30,14	77		E	1	Fine.
23	7	0	64	67	30,08	63		E	1	Cloudy and hazy. [Sun.
	2	0	73	77	30,03	73		E	1	Fine. Therm. 83° in the
24	7	0	64	68	29,95	61		E	1	Cloudy and hazy.
	2	0	76	79	29,91	77		E	1	Fine.
25	7	0	66	70	29,90	64		E	1	Cloudy.
	2	0	74	74	29,90	77		S	1	Fine.
26	7	0	63	65	29,94	62		N	1	Cloudy; rain in the night.
	2	0	71	71	29,98	76		SE	1	Fine.
27	7	0	62	64	30,11	58		E	1	Dull and Cloudy.
	2	0	63	64	30,11	63		E	1	Cloudy.
28	7	0	59	62	29,95	57		E	1	Cloudy.
	2	0	60	63	29,87	63		E	1	Cloudy.
29	7	0	57	63	29,76	54		E	1	Cloudy.
	2	0	58	63	29,73	59		E	1	Rain.
30	7	0	60	63	29,71	56		WNW	1	Cloudy.
	2	0	69	63	29,71	71		SSE	1	Fine.
31	7	0	62	65	29,67	61		SW	1	Cloudy.
	2	0	68	67	29,67	70		W	1	Rain.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Sept. 1	7	0	62	65	29,85	61	Rain this Month 1,810 Inches.	W	1	Cloudy and hazy.
	2	0	68	67	29,91	69		SW	1	Fine.
2	7	0	63	65	30,02	61		W	1	Cloudy.
	2	0	68	70	30,04	70		NW	1	Fine.
3	7	0	62	65	29,96	62		S	1	Cloudy.
	2	0	70	69	29,91	72		SWbyS	1	Cloudy.
4	7	0	65	67	29,84	63		SW	1	Cloudy.
	2	0	70	69	29,77	72		W	1	Fine.
5	7	0	63	65	29,77	60		W	2	Fine.
	2	0	68	70	29,99	71		SW	1	Fine.
6	7	0	65	66	29,92	61		S by E	1	Fine.
	2	0	71	71	29,93	73		SW	1,2	Cloudy.
7	7	0	67	67	29,66	67		S by E	1	Cloudy.
	2	0	71	68	29,61	75		SW	1	Fine.
8	7	0	61	65	29,59	60		W	1	Cloudy.
	2	0	65	70	29,67	71		W	1	Fine.
9	7	0	61	64	29,70	55		S	1	Fine.
	2	0	68	65	29,57	69		SSE	1	Cloudy.
10	7	0	57	63	29,66	53		WbyN	1	Fine.
	2	0	64	68	29,74	68		NNW	1	Fine.
11	7	0	57	62	29,94	53		W	1	Fine.
	2	0	64	67	29,99	65		WSW	1	Fine.
12	7	0	56	62	29,73	59		SW	1	Rain.
	2	0	65	66	29,62	67		W	1	Rain.
13	7	0	57	61	29,90	54		W	1	Cloudy.
	2	0	65	64	29,95	66		SSW	1	Cloudy, but pleasant.
14	7	0	54	62	29,92	54		SW	1	Cloudy.
	2	0	65	62	29,98	59		N by E	1	Cloudy.
15	7	0	51	59	30,18	50		E	1	Foggy.
	2	0	65	64	30,20	65		NW	1	Fine.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Sept. 16	7	0	62	63	30,20	61	Rain this Month 1,810 Inches.	W	1	Cloudy, rather hazy.
	2	0	69	66	30,19	70		W	1	Fine.
17	7	0	63	64	30,13	61		W	1	Cloudy.
	2	0	70	68	30,06	71		NW	1	Cloudy.
18	7	0	63	65	29,96	61		S	1	Fine.
	2	0	69	68	29,87	70		NW	1	Fine.
19	7	0	58	62	29,84	55		W	1	Fine.
	2	0	66	65	29,90	68		N	1	Cloudy.
20	7	0	54	60	29,95	50		W	1	Cloudy.
	2	0	62	61	29,84	64		W	1	Cloudy.
21	7	0	59	62	29,70	54		S	1	Cloudy.
	2	0	65	64	29,68	66		W	1	Cloudy.
22	7	0	60	62	29,72	58		N	1	Fine; a storm of thunder and lightning in the night.
	2	0	66	64	29,74	67		E	1	Fine.
23	7	0	61	63	29,69	60		E	1	Cloudy.
	2	0	66	63	29,66	67		E	1	Cloudy and hazy.
24	7	0	57	60	29,62	53		WSW	1	Fine.
	2	0	64	63	29,76	65		W	1	Cloudy.
25	7	0	52	59	29,95	49		W	1	Fine, rather cloudy.
	2	0	65	61	29,98	67		W	1	Fine.
26	7	0	60	62	29,97	52		W	1	Cloudy.
	2	0	67	65	29,96	68		W	1	Cloudy.
27	7	0	59	61	29,85	55		W	1	Rainy dark weather.
	2	0	63	64	29,89	67		W	1	Cloudy.
28	7	0	55	60	29,92	52		W	1	Cloudy.
	2	0	63	63	29,75	65		E	1	Fine.
29	7	0	54	57	29,49	53		SW	2	Cloudy; a violent gale in the night.
	2	0	60	60	29,53	63		E	1	Fine.
30	7	0	49	58	29,78	46		W	1	Fine.
	2	0	58	59	29,85	60		NW	1	Cloudy.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Winds. Points.	Winds. Str.	Weather.
Oct. 1	8	0	58	59	29,67	50	NW	1	Cloudy.
	2	0	63	62	29,70	64	NW	1,2	Cloudy.
2	8	0	52	57	30,08	48	NNW	1	Fine.
	2	0	58	62	30,14	65	W	1	Fine.
3	8	0	58	60	29,99	52	SW	1	Cloudy.
	2	0	65	62	29,87	66	W	1,2	Cloudy.
4	8	0	59	62	29,65	58	S	1	Rain.
	2	0	61	62	29,49	65	ESE	1	Rain.
5	8	0	49	57	29,79	47	NW	1	Fine.
	2	0	56	61	29,93	62	NW	1	Fine.
6	8	0	49	57	30,02	47	W	1	Fine.
	2	0	60	60	30,03	61	W	1	Fine.
7	8	0	57	59	30,04	50	W	1	Fine.
	2	0	61	60	30,03	63	W	1	Cloudy.
8	8	0	56	59	29,92	53	S	2	Rain.
	2	0	58	62	30,03	61	N	1	Fine.
9	8	0	48	56	30,26	45	E	1	Fine.
	2	0	66	62	30,24	68	E	1	Fine.
10	8	0	51	55	30,09	47	E	1	Fine.
	2	0	59	63	29,98	59	E	1	Fine.
11	8	0	49	57	29,76	48	E	1	Cloudy.
	2	0	54	58	29,64	58	E	1	Rain.
12	8	0	51	56	29,71	50	NW	1	Hazy and Cloudy.
	2	0	56	58	29,88	56	SW	1	Fine.
13	8	0	50	56	30,26	49	N	1	Fine, rather hazy.
	2	0	56	56	30,31	57	SW	1	Cloudy.
14	8	0	45	54	29,33	42	E	1	Hazy.
	2	0	58	67	30,28	58	WSW	1	Cloudy.
15	8	0	50	56	30,23	45	N	1	Rain.
	2	0	58	58	30,20	53	NE	1	Fine.
16	8	0	44	54	30,16	42	W	1	Fine, rather hazy.
	2	0	54	57	30,14	52	N	1	Fine.

Rain this Month 1,931 Inches.

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for October, 1821.

1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Oct. 17	8	0	46	54	30,15	45	Rain this Month 1,931 Inches.	NW	1	Cloudy.
	2	0	53	55	30,07	51		W	1	Cloudy.
18	8	0	49	54	29,92	48		W	1	Rain.
	2	0	59	60	29,89	60		W	1	Cloudy.
19	8	0	50	55	29,89	49		W	1	Cloudy.
	2	0	54	53	29,79	58		W	1	Cloudy.
20	S	0	51	53	29,27	48		S	2	Cloudy.
	2	0	53	55	28,92	56		NW	1	Rain.
21	8	0	43	53	29,11	43		W	1	Fine.
	2	0	49	56	29,17	53		WSW	1	Fine.
22	8	0	45	53	29,12	43		S	1	Cloudy.
	2	0	53	56	29,21	54		S	1,2	Fine.
23	8	0	47	53	29,34	45		S	2	Cloudy.
	2	0	50	53	29,21	52		E	1	Rain.
24	8	0	44	53	29,38	43		S	1	Fine.
	2	0	53	55	29,52	53		NW	1	Rain.
25	8	0	43	53	29,91	41		W	1	Cloudy.
	2	0	53	59	29,94	54		W	1	Rain.
26	8	0	50	56	30,06	43		W	1	Damp and hazy.
	2	0	58	59	30,09	58		S	1	Cloudy.
27	8	0	55	57	30,14	53		S by E	1	Cloudy.
	2	0	59	61	30,17	60		W	1	Cloudy.
28	8	0	55	58	30,25	54		SSW	1	Cloudy.
	2	0	60	58	30,25	61		SW	1	Cloudy and hazy.
29	8	0	43	56	30,26	42		W	1	Thick fog.
	2	0	60	60	30,20	61		E	1	Fine, rather hazy.
30	8	0	45	56	30,11	42		E	1	Hazy.
	2	0	54	59	30,04	55		SW	1	Fine.
31	8	0	47	54	29,88	44		SE	1	Hazy.
	2	0	57	58	29,91	59		W	1	Fine.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Nov. 1	8	0	55	57	29,89	51	Rain this Month 3,445 Inches.	S	1	Cloudy.
	2	0	59	59	29,89	60		SW	1	Cloudy.
2	8	0	59	59	29,90	57		W	2	Cloudy.
	2	0	62	62	29,85	63		W	1	Cloudy.
3	8	0	55	60	29,80	54		W	1	Cloudy.
	2	0	57	56	29,77	63		SW	1	Rain.
4	8	0	44	55	29,21	42		NW	1,2	Fine; stormy in the night
	2	0	45	50	29,99	56		W	1	Cloudy.
5	8	0	35	42	30,03	34		W	1	Fine.
	2	0	44	48	30,12	45		W	1	Cloudy and hazy.
6	8	0	37	47	30,15	35		W	1	Fine.
	2	0	45	48	30,20	45		W	1	Fine.
7	8	0	42	48	30,13	37		E	1	Fine.
	2	0	48	49	30,08	49		SE	1	Fine.
8	8	0	42	49	30,05	42		E	1	Fine, rather hazy.
	2	0	48	55	30,05	49		E	1	Cloudy.
9	8	0	41	53	30,01	40		E	1	Fine.
	2	0	45	54	29,99	47		E	1	Fine.
10	8	0	44	53	29,97	41		E	1	Foggy.
	2	0	52	58	30,07	53		E	1	Foggy.
11	8	0	50	57	29,86	44		SSE	1	Fine.
	2	0	55	56	29,89	57		SE	1	Rain.
12	8	0	49	55	29,81	48		W	1	Fine.
	2	0	54	63	29,92	56		E	1	Fine.
13	8	0	46	57	29,99	44		E	1	Foggy.
	2	0	54	60	29,87	54		E	1	Cloudy.
14	8	0	53	59	29,71	46		W	1	Hazy.
	2	0	59	61	29,78	56		W	1	Cloudy.
15	8	0	53	60	29,74	50		S	1	Fine.
	2	0	60	63	29,66	61		S	1,2	Cloudy.
16	8	0	55	61	29,51	52		SW	2	Cloudy.
	2	0	54	62	29,41	60		W	1	Cloudy. A violent storm of wind and rain from 10 A M. to 3 P. M.

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1821	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Nov. 17	8	0	52	63	29,53	49	Rain this Month 3,445 Inches.	W	1	Fine.
	2	0	56	60	29,68	57		W	1	Cloudy.
18	8	0	51	60	29,83	50		W	1	Cloudy.
	2	0	59	59	30,02	59		W	1	Cloudy.
19	8	0	50	58	29,99	47		S	1	Cloudy.
	2	0	51	61	29,84	54		SW	1	Fine.
20	8	0	46	58	30,01	44		W	1,2	Fine.
	2	0	51	59	29,75	52		W	1	Cloudy.
21	8	0	51	59	29,61	46		W	1	Cloudy and hazy.
	2	0	48	61	29,75	53		NW	1	Fine.
22	8	0	44	55	29,61	41		S	1,2	Rain.
	2	0	57	62	29,47	58		W	1,2	Fine.
23	8	0	52	59	29,73	45		W	2	Cloudy.
	2	0	48	61	29,81	56		W	1	Fine.
24	8	0	44	56	29,79	40		SW	1	Rain.
	2	0	50	58	29,67	52		W	1	Cloudy.
25	8	0	45	56	29,73	44		W	1	Fine.
	2	0	48	56	29,77	51		W	1	Cloudy.
26	8	0	45	57	29,36	54		S	2	Cloudy.
	2	0	54	60	29,22	56		W	3	Stormy.
27	8	0	45	56	29,43	45		W	1	Cloudy.
	2	0	46	57	29,60	50		W	1	Fine.
28	8	0	40	53	29,81	36		SW	1	Fine.
	2	0	50	57	29,63	55		W	1	Cloudy.
29	8	0	53	57	29,57	41		WSW	3	Rain.
	2	0	51	62	29,74	55		W	1	Fine.
30	8	0	43	55	29,78	43		W	1	Fine.
	2	0	49	58	29,91	52		SW	1	Cloudy and hazy.

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for December, 1821.

1821	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Dec. 1	8	0	44	55	29,56	43	Rain this Month 3,936 Inches.	E	2	Fine, a violent gale of wind [in the night.
	2	0	46	58	29,64	55		W	1	Fine. [ning in the night.
2	8	0	44	53	29,81	41		W	1	Cloudy, wind and light-
	2	0	48	56	29,87	49		W	1	Fine.
3	8	0	50	53	29,75	40		S	2	Cloudy.
	2	0	54	58	29,59	58		NW	1,2	Rain.
4	8	0	39	52	29,86	37		W	1	Fine.
	2	0	56	57	29,87	57		W	1	Cloudy.
5	8	0	48	55	29,74	39		W	2	Rain. [ning at 3 P.M.
	2	0	58	60	29,80	48		W	1,2	Rain, thunder and light-
6	8	0	39	53	30,12	38		W	1	Hazy.
	2	0	43	56	30,19	48		W	1	Fine.
7	8	0	40	53	30,10	38		SE	1	Cloudy.
	2	0	45	56	29,90	45		SE	1	Cloudy.
8	8	0	48	55	29,96	43		W	1	Cloudy.
	2	0	51	57	30,03	52		W	1	Cloudy.
9	8	0	48	56	30,08	44		SW	1	Cloudy.
	2	0	52	56	30,08	54		W	1	Rain.
10	8	0	48	56	30,05	44		S	2	Cloudy.
	2	0	53	59	29,98	54		S	2	Fine.
11	8	0	43	55	30,17	43		W	1	Foggy.
	2	0	46	59	30,26	54		N	1	Fine.
12	8	0	42	53	30,28	37		E	1	Fine.
	2	0	48	56	30,21	48		SSE	1	Cloudy.
13	8	0	47	54	29,95	45		S by E	1	Cloudy.
	2	0	52	59	29,92	53		S by E	1	Fine.
14	8	0	43	56	30,00	42		S	1	Fine.
	2	0	51	56	29,98	54		SW	1	Rain.
15	8	0	49	56	29,96	43		S	1	Fine.
	2	0	51	60	29,96	53		SSW	1	Fine.
16	8	0	50	55	29,80	49		S	1	Fine.
			53	57	29,73	54		S	1	Cloudy.

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1821	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Dec. 17	8	0	50	53	29,51	49	Rain this Month 3,936 Inches.	SW	2,3	Rain.
	2	0	52	55	29,49	53		SW	1	Cloudy. [at 3 A.M.
18	8	0	45	55	28,98	43		W	1	Fine, a most violent gale
	2	0	50	58	29,13	53		W	2	Rain and hail.
19	8	0	45	54	29,03	44		SW	1	Fine.
	2	0	49	57	29,12	50		NW	1	Fine.
20	S	0	42	53	29,31	40		W	1	Foggy.
	2	0	46	57	29,37	51		W	1	Fine.
21	8	0	47	54	28,80	40		NW	2	Cloudy.
	2	0	49	52	29,12	50		W	2	Fine.
22	8	0	41	53	29,41	41		SW	1	Fine.
	2	0	49	57	29,48	50		W	1	Cloudy.
23	8	0	42	52	28,97	41		W	2	Fine.
	2	0	47	52	29,15	52		W	1	Rain.
24	8	0	43	51	28,92	41		S	2	Rain.
	2	0	47	54	28,75	48		E	1	Rain.
25	8	0	40	52	28,18	40		S	1	Cloudy.
	2	0	42	53	28,46	48		W	1	Fine.
26	8	0	37	49	28,51	36		E	1	Hazy.
	2	0	42	53	28,48	45		E	1	Cloudy.
27	8	0	39	48	28,91	35		S	1	Fine.
	2	0	45	51	28,98	46		S	1,2	Fine.
28	8	0	40	49	29,85	39		S	1,2	Cloudy. [wind and rain.
	2	0	44	49	28,45	46		E	3	Rain, a violent squall of
29	8	0	44	50	28,30	37		E	1	Thick fog.
	2	0	48	53	28,56	48		W	1	Cloudy.
30	8	0	42	50	29,01	42		W	1	Cloudy.
	2	0	44	50	29,15	47		NW	1	Rain.
31	8	0	37	48	29,84	37		W	1	Fine.
	2	0	44	53	29,96	45		W	1	Fine.

1821.	Thermometer without.			Thermometer within.			Barometer.*			Six's Thermometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	51	25	39,5	58	38	49,0	30,76	28,98	30,01	52	25	39,7	1,854
February	59	25	38,0	59	42	50,3	30,77	29,21	30,23	61	25	38,4	0,278
March	57	34	44,7	69	48	56,4	30,38	29,06	29,63	59	31	45,2	1,917
April	71	37	53,5	69	51	58,6	30,12	29,25	29,65	72	36	53,1	1,137
May	66	42	53,3	65	52	57,8	30,25	29,17	29,86	70	37	52,7	1,093
June	72	47	57,7	67	52	59,4	30,32	29,57	30,03	73	43	57,0	1,833
July	72	51	60,9	71	57	62,4	30,30	29,56	29,91	74	46	60,6	2,236
August	76	56	65,3	79	61	66,6	30,19	29,47	29,92	77	50	64,7	2,097
September	71	49	62,4	71	57	64,0	30,20	29,49	29,85	75	46	62,0	1,810
October	66	43	53,3	67	53	57,4	30,33	29,11	29,87	68	41	52,7	1,931
November	62	35	49,7	63	42	56,8	30,20	29,21	29,80	63	34	49,6	3,445
December	58	37	46,1	60	49	54,3	30,28	28,18	29,51	58	36	45,8	3,936
Whole year			52,0			57,8			29,86			51,8	23,567

* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75,6 inches above the surrounding ground.

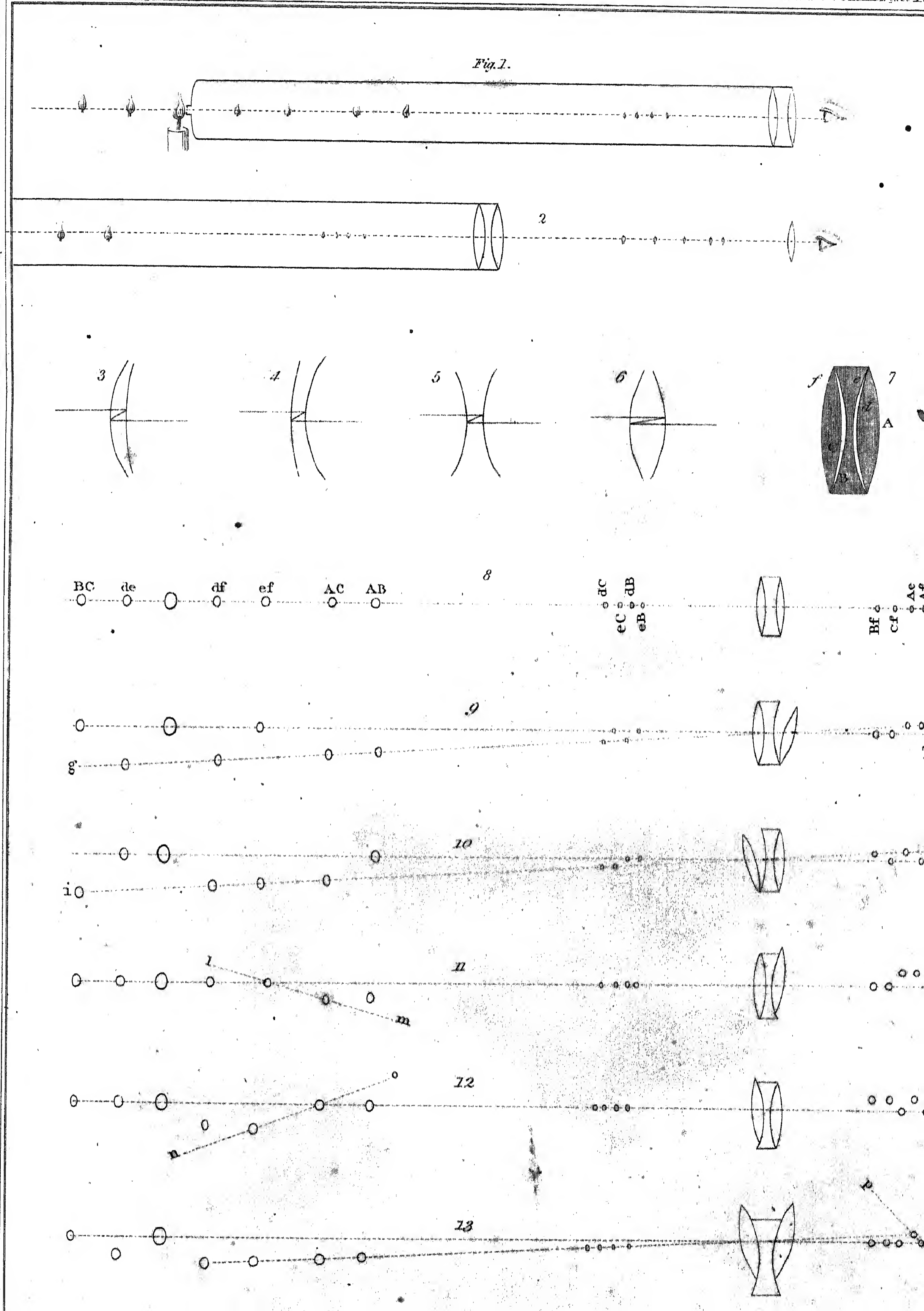
Fig. 1.
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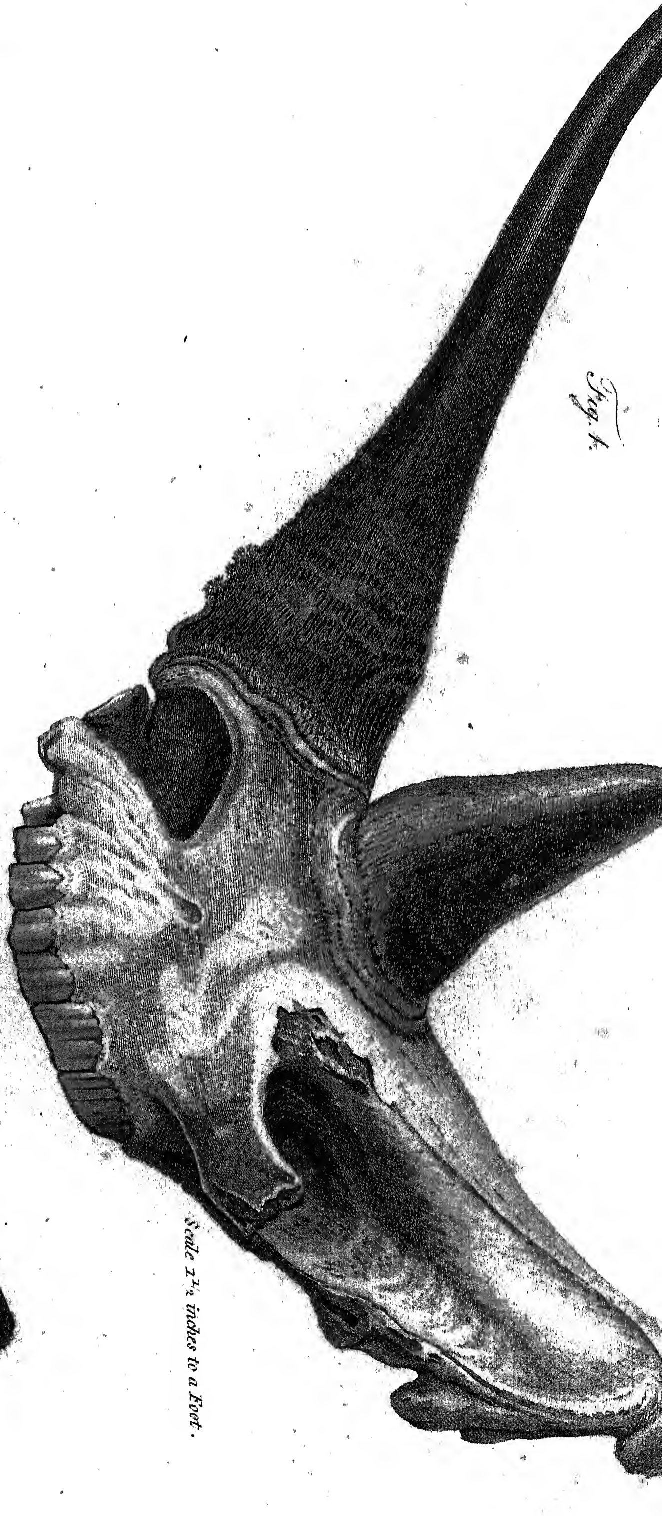


Fig. 1.

Scale 2½ inches to a Foot.

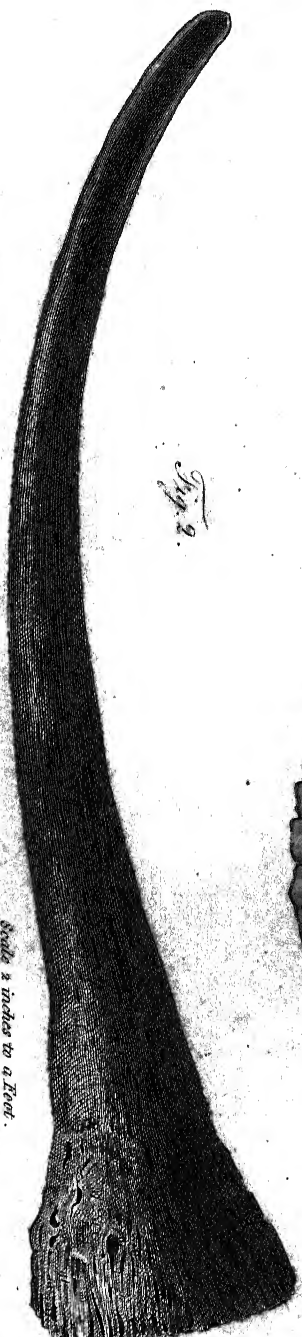
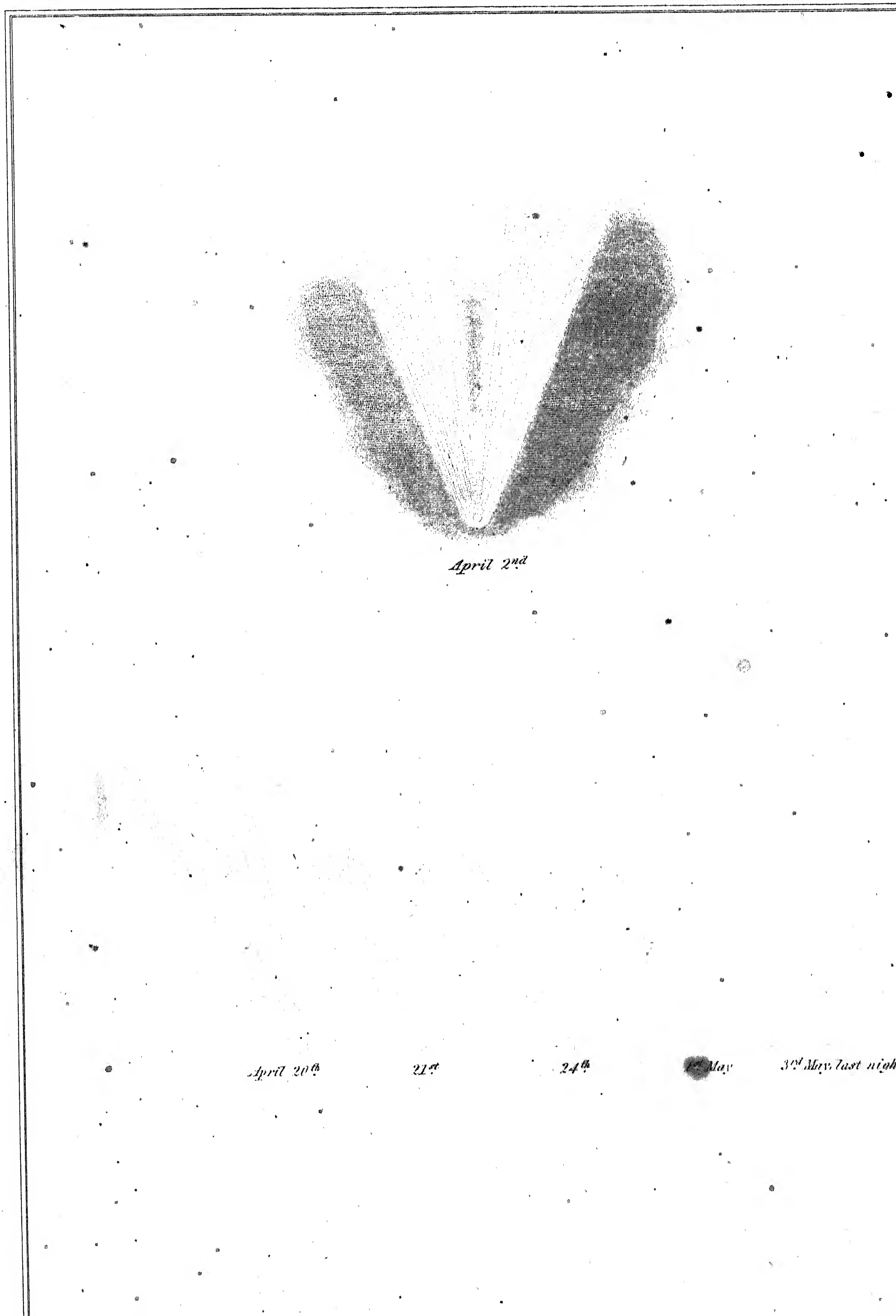


Fig. 2.

Scale 2 inches to a Foot.

Scale 3 inches to a Foot.


April 2nd
April 20th
21st
24th
1st May
3rd May, last nigh

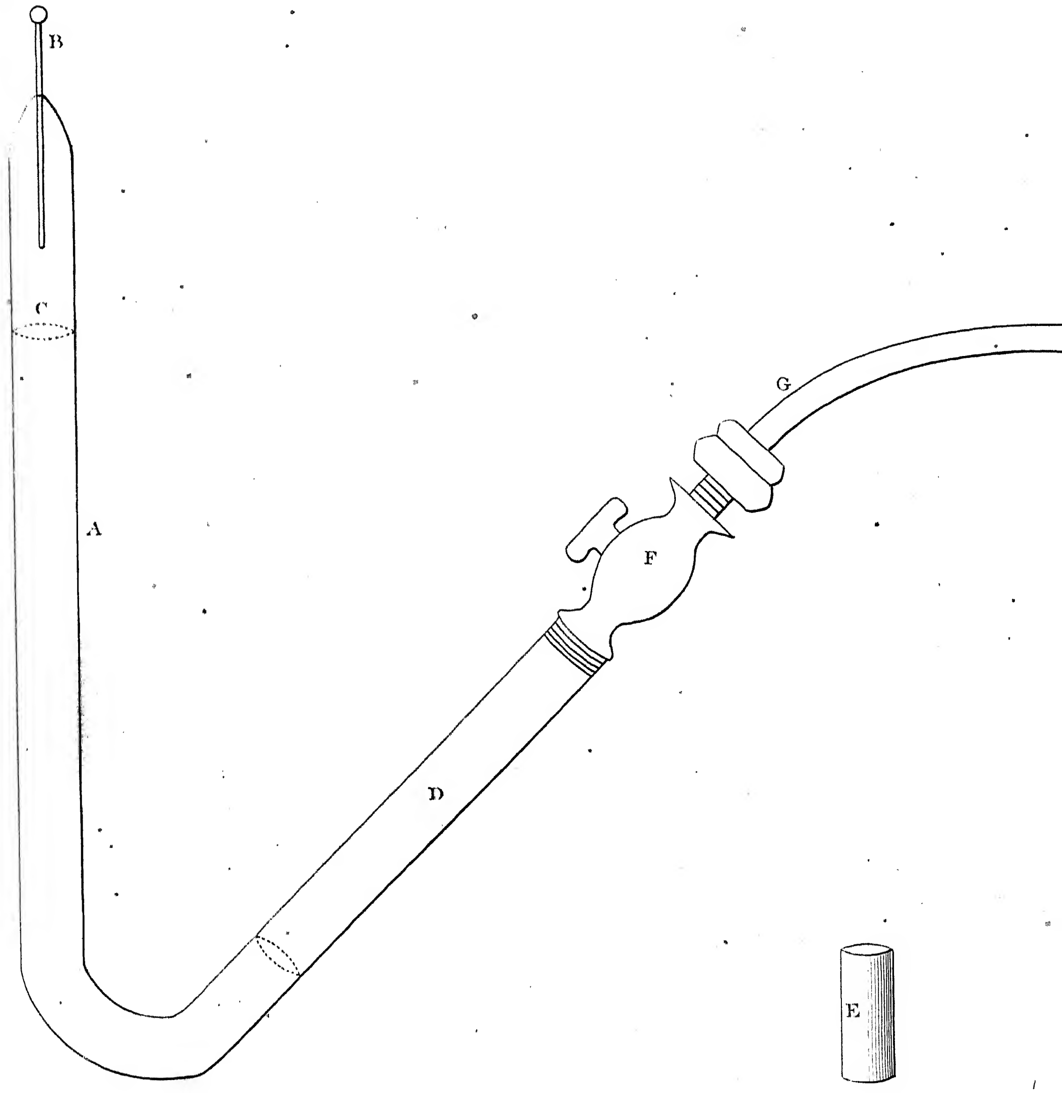

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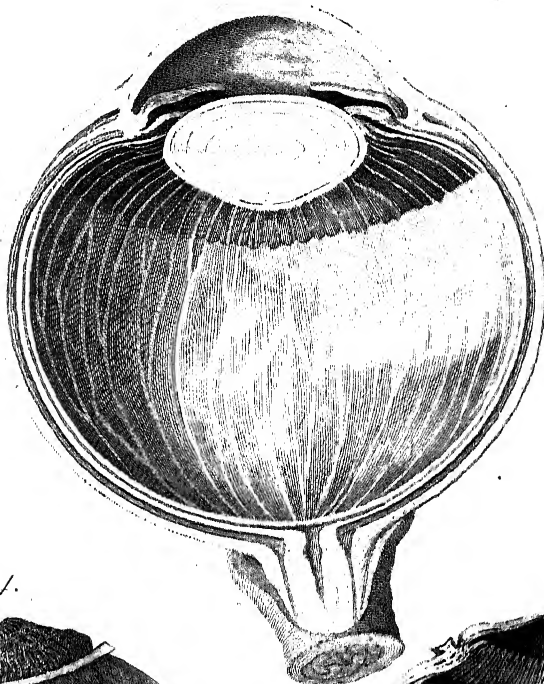
Fig. 3.



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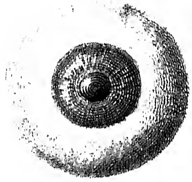

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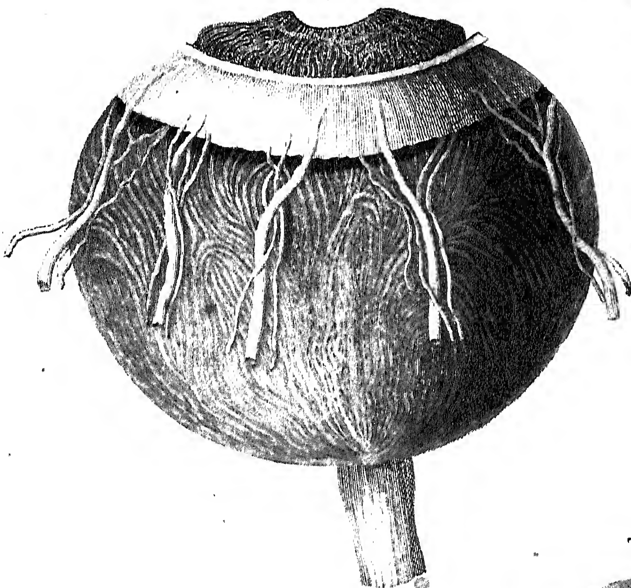

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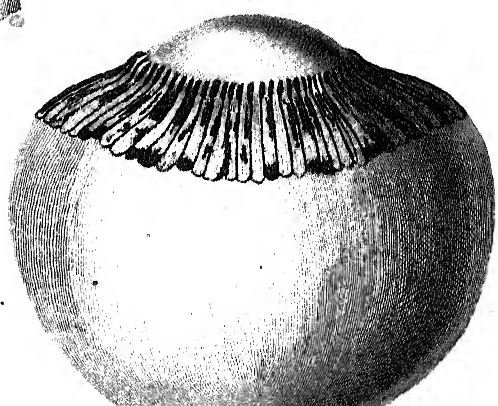

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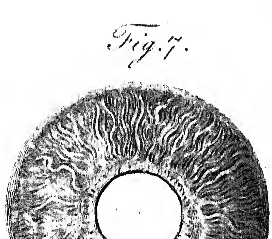

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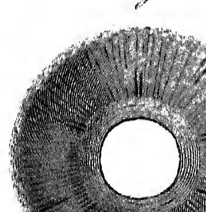

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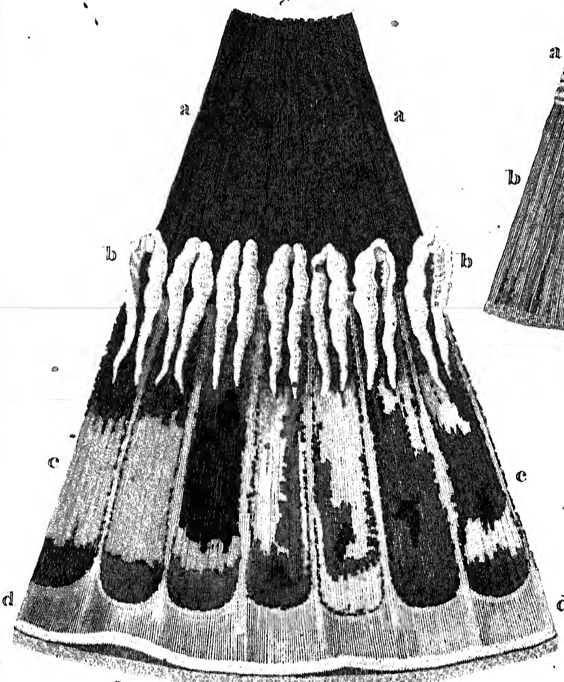

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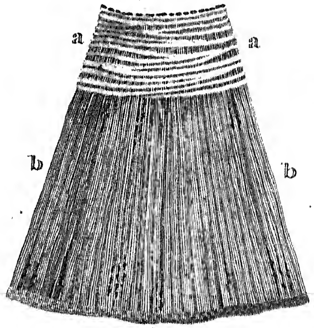

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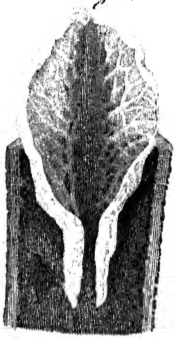

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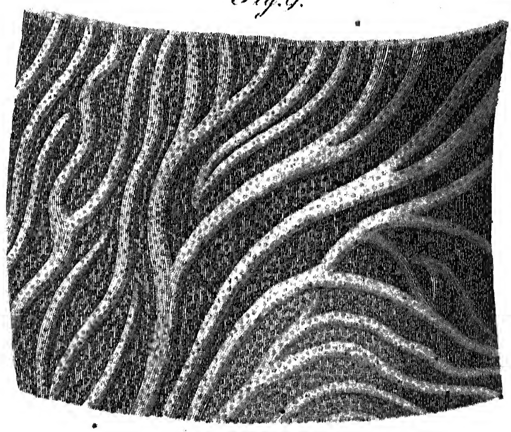

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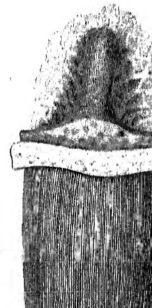

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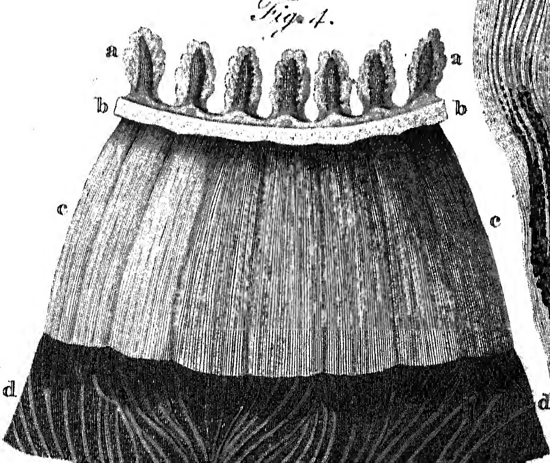

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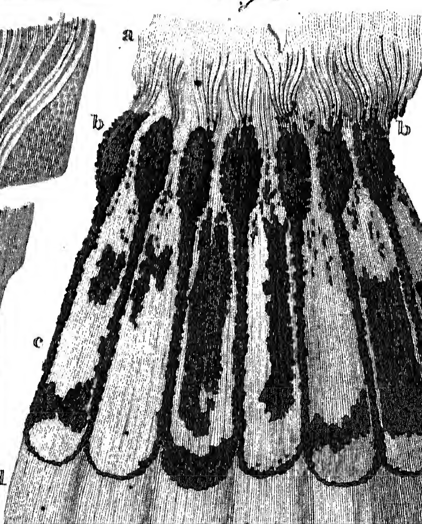

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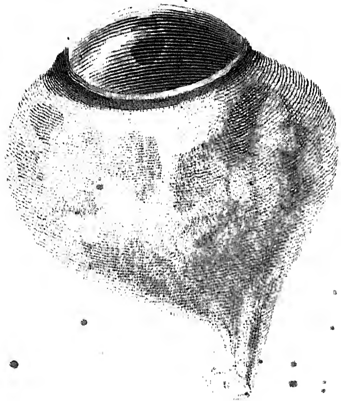

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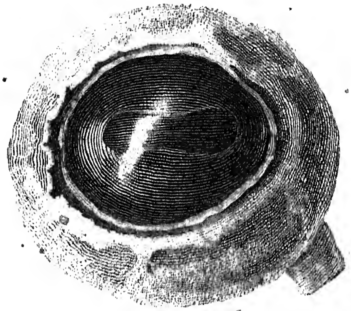

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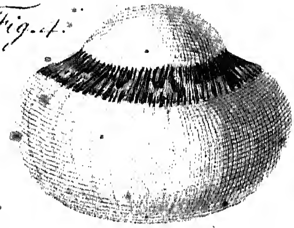

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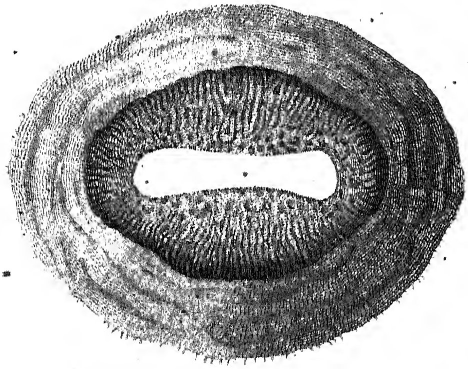

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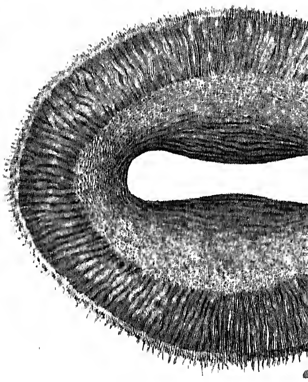

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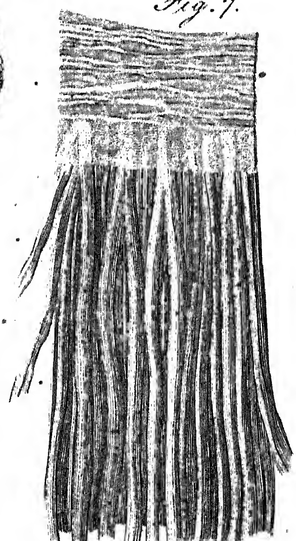

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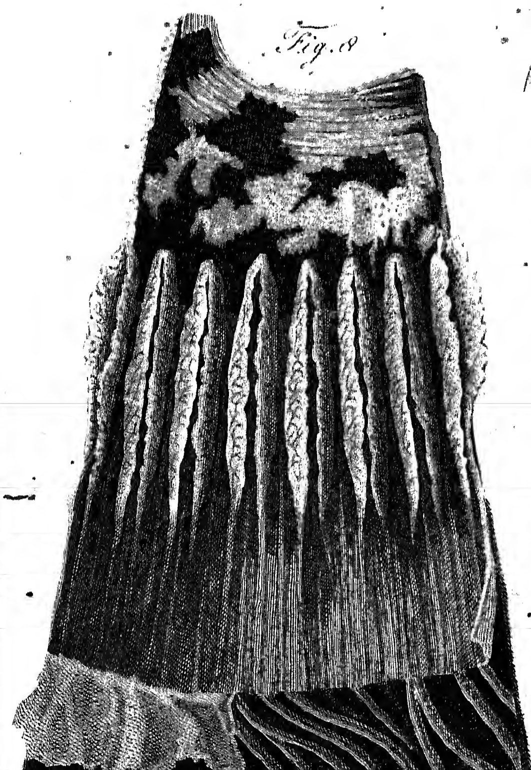

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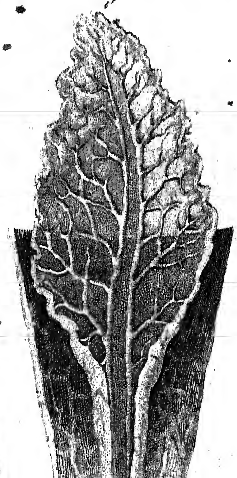

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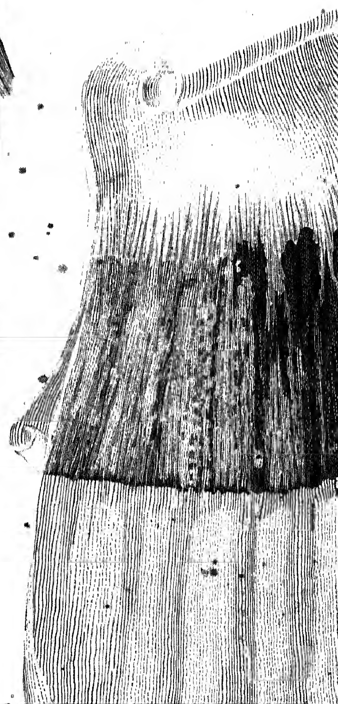

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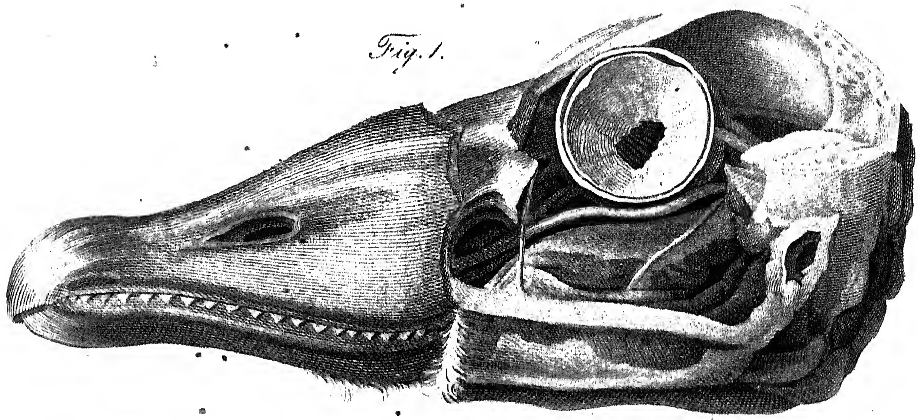

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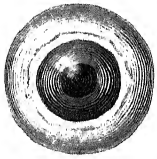

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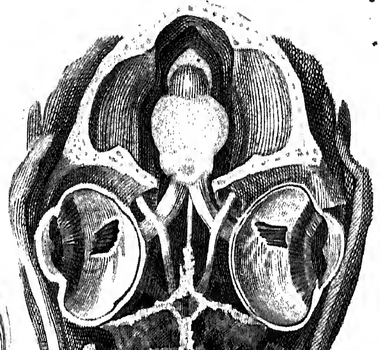

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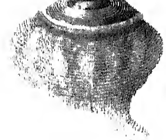

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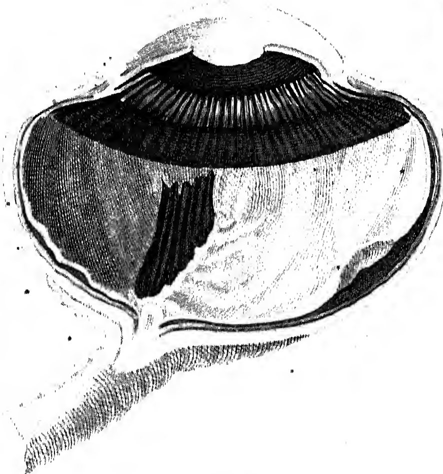

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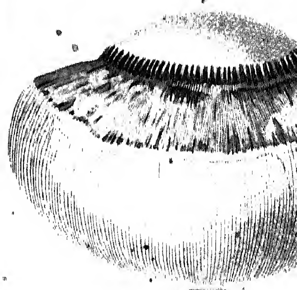

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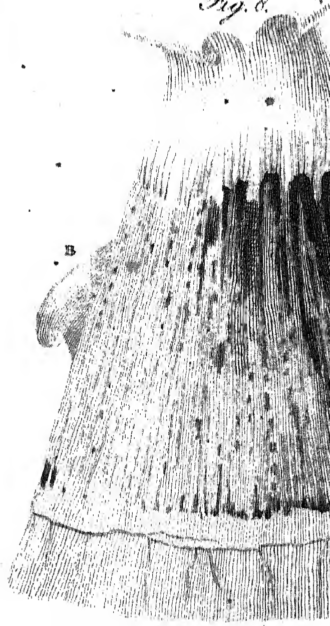

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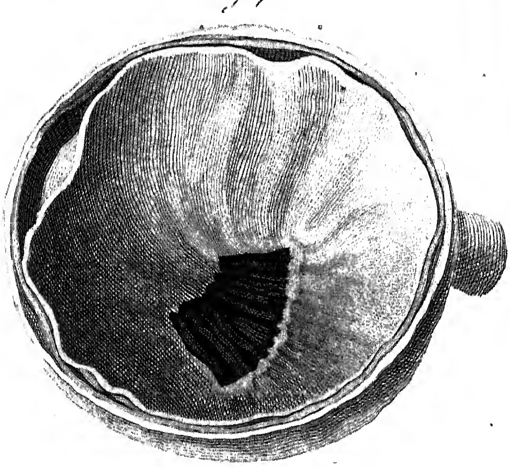
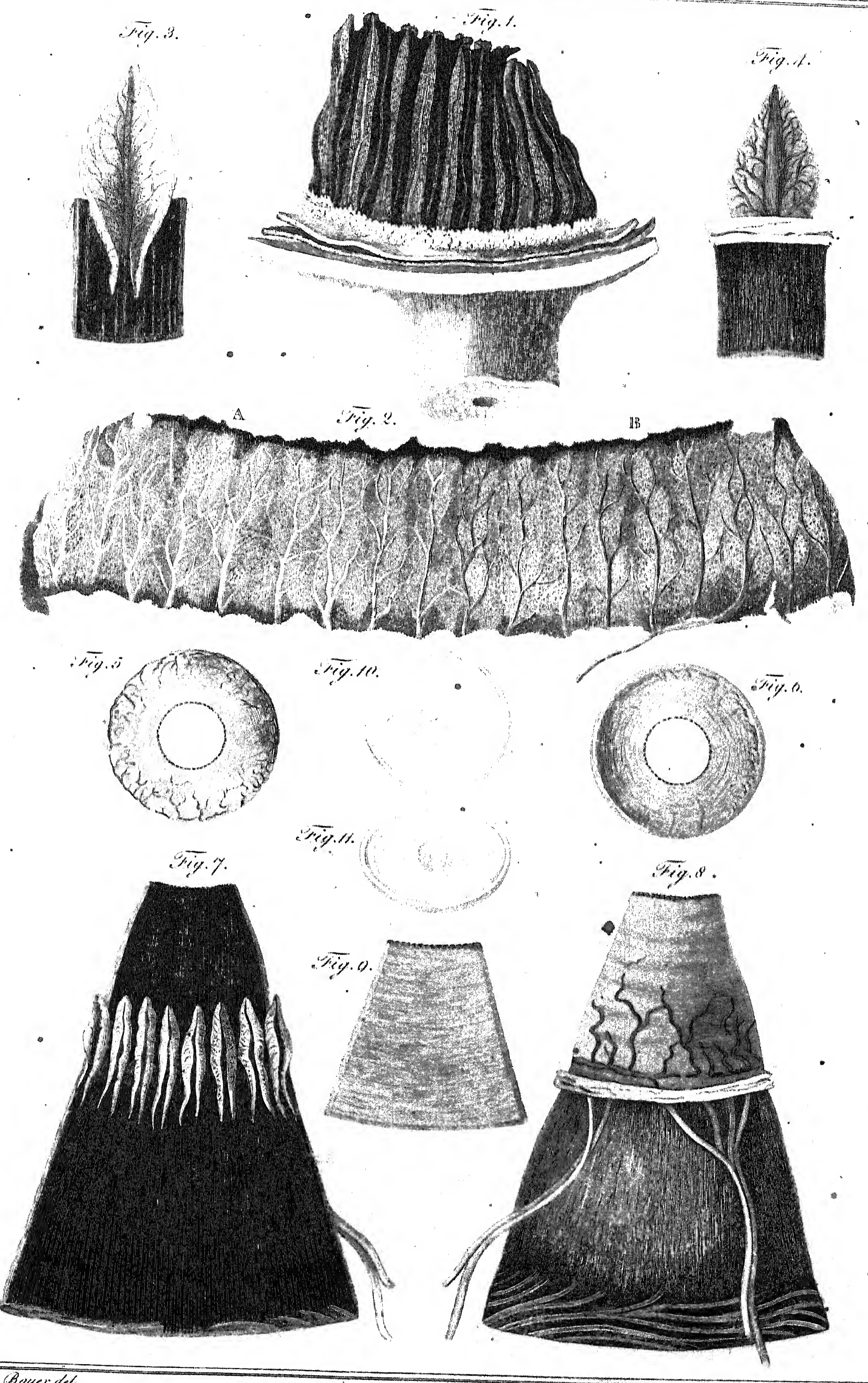
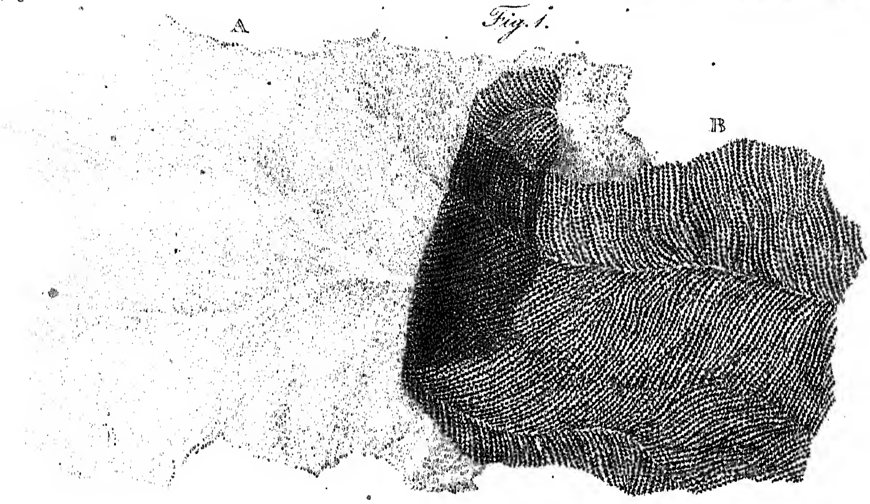




Franz Bauer del.

J.s Basire sc.

Fig. 1.



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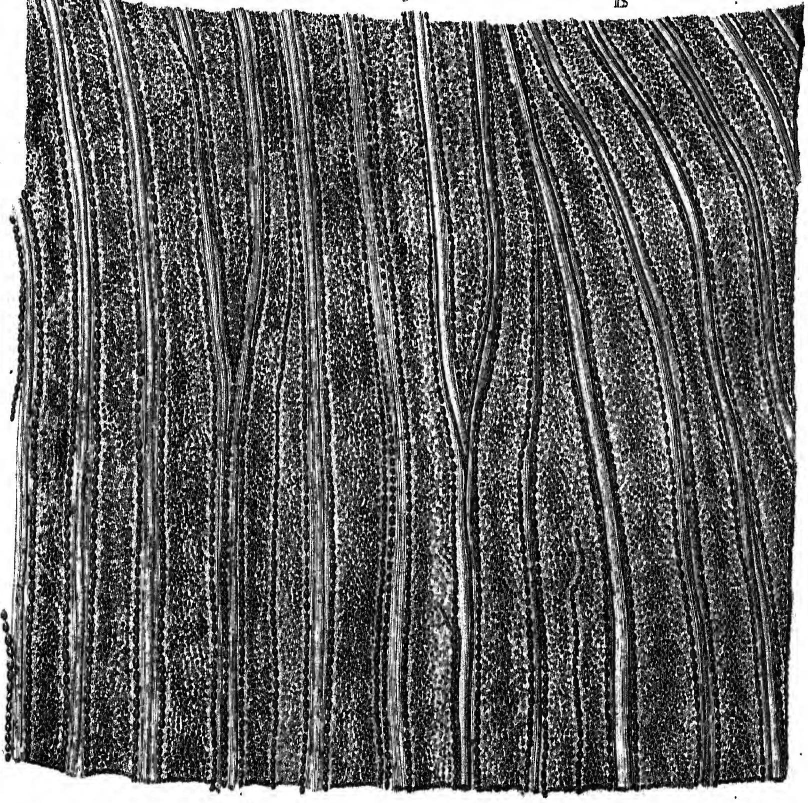

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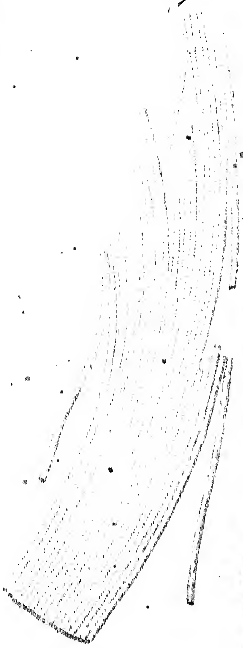

Fig. 3.

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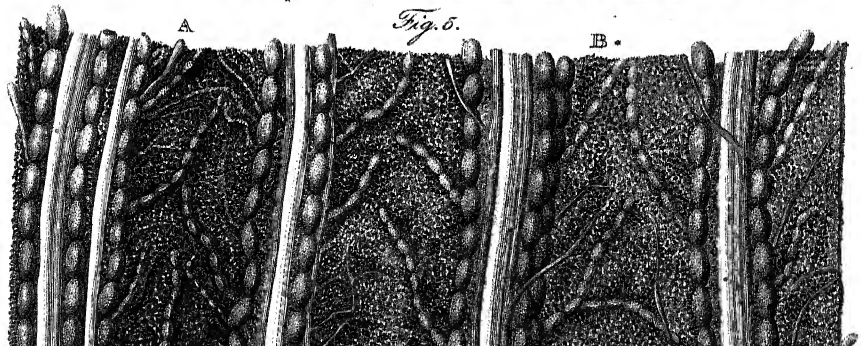

Franz Bauer del.

J. Basire sc.

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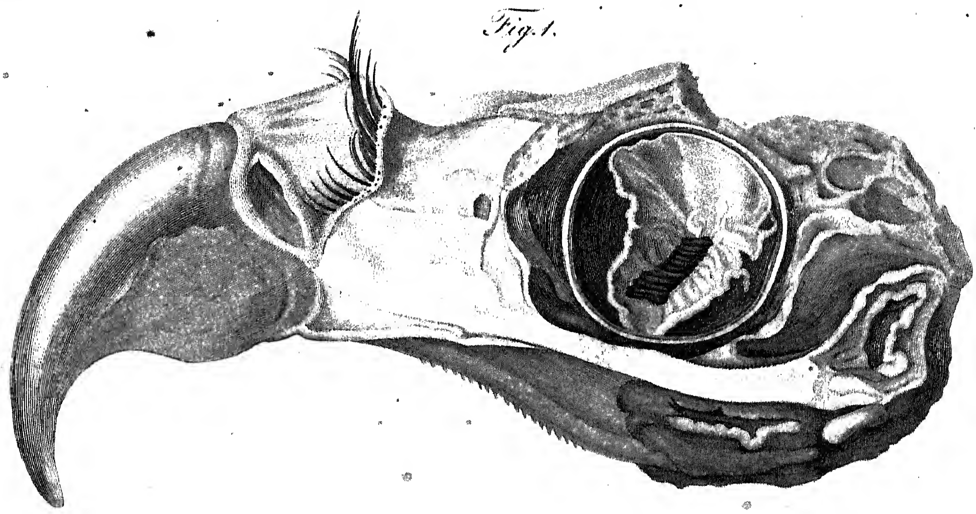

Fig. 2.

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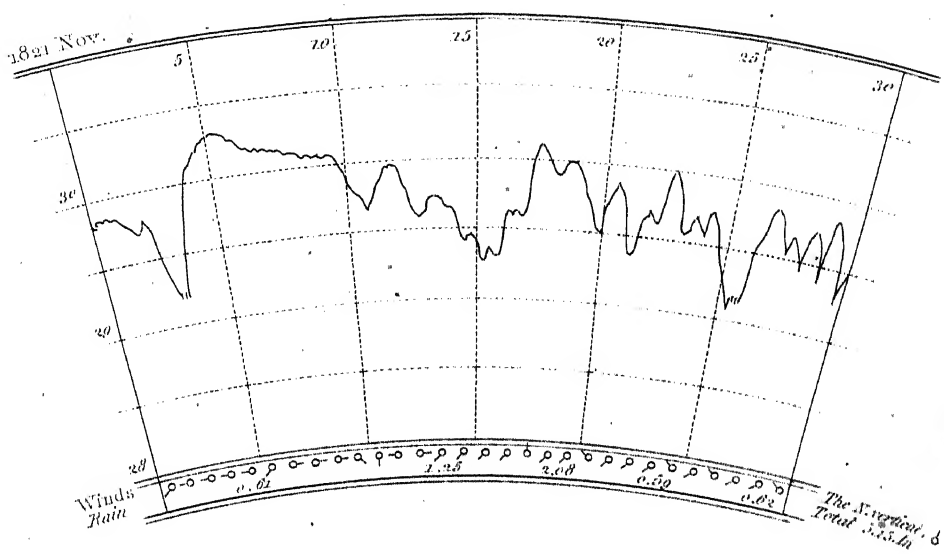
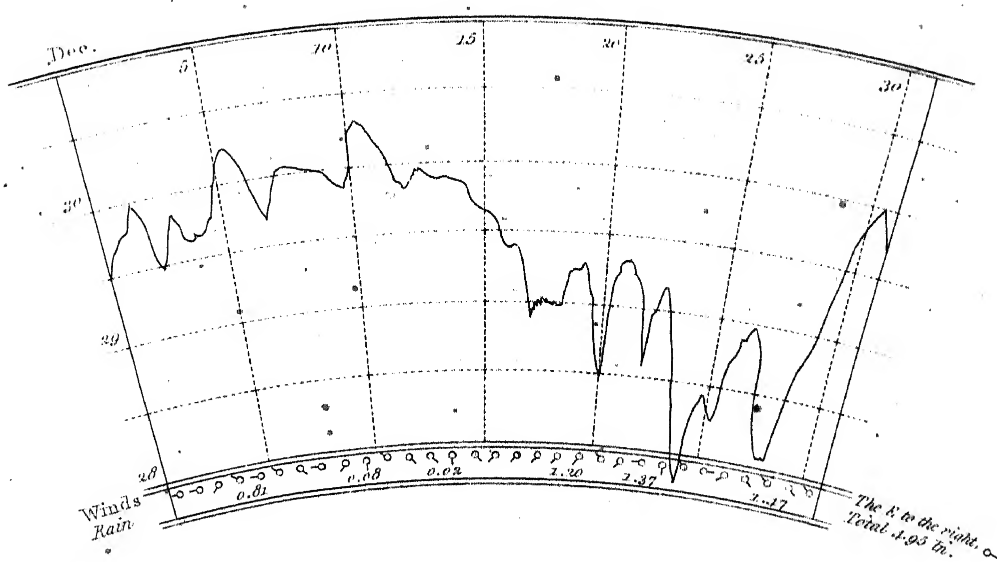
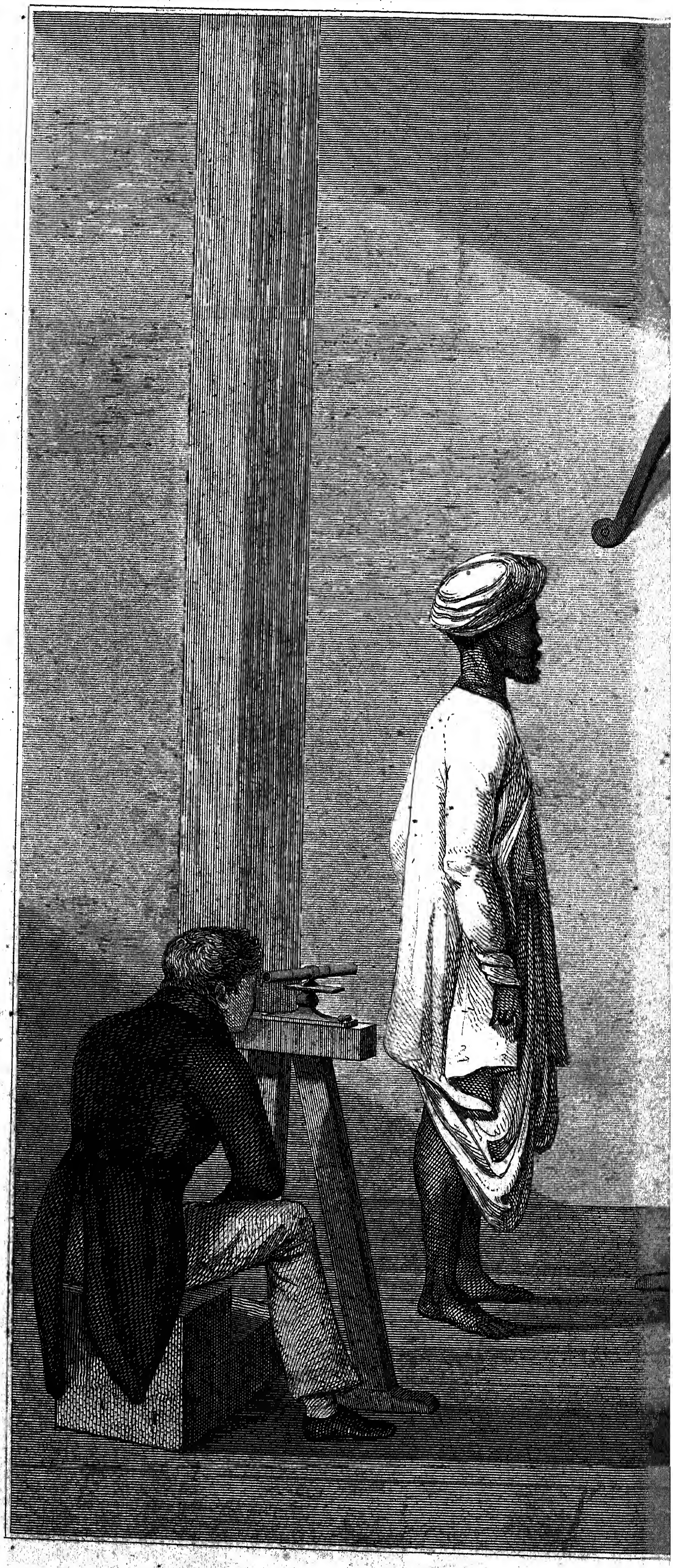
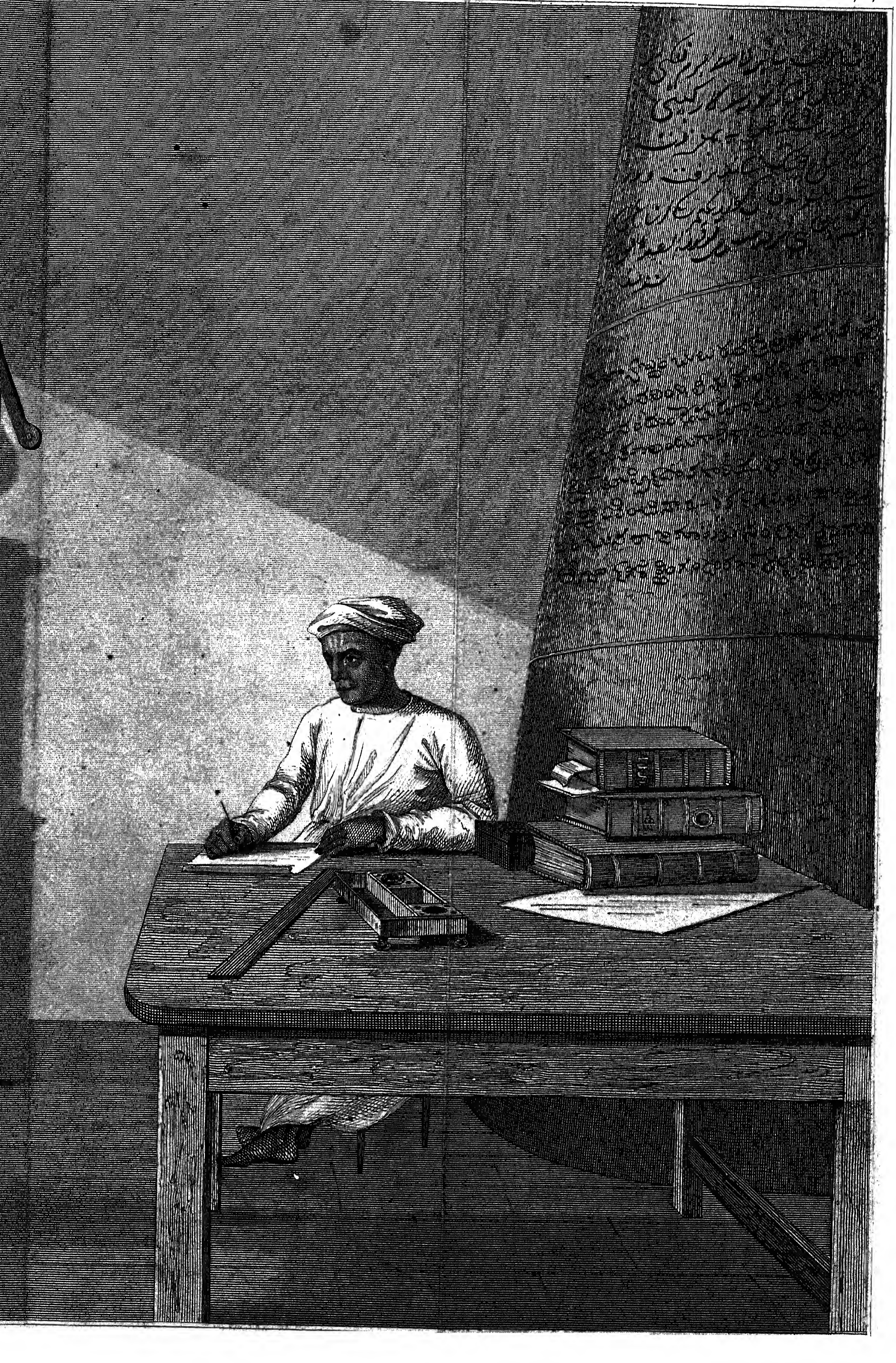
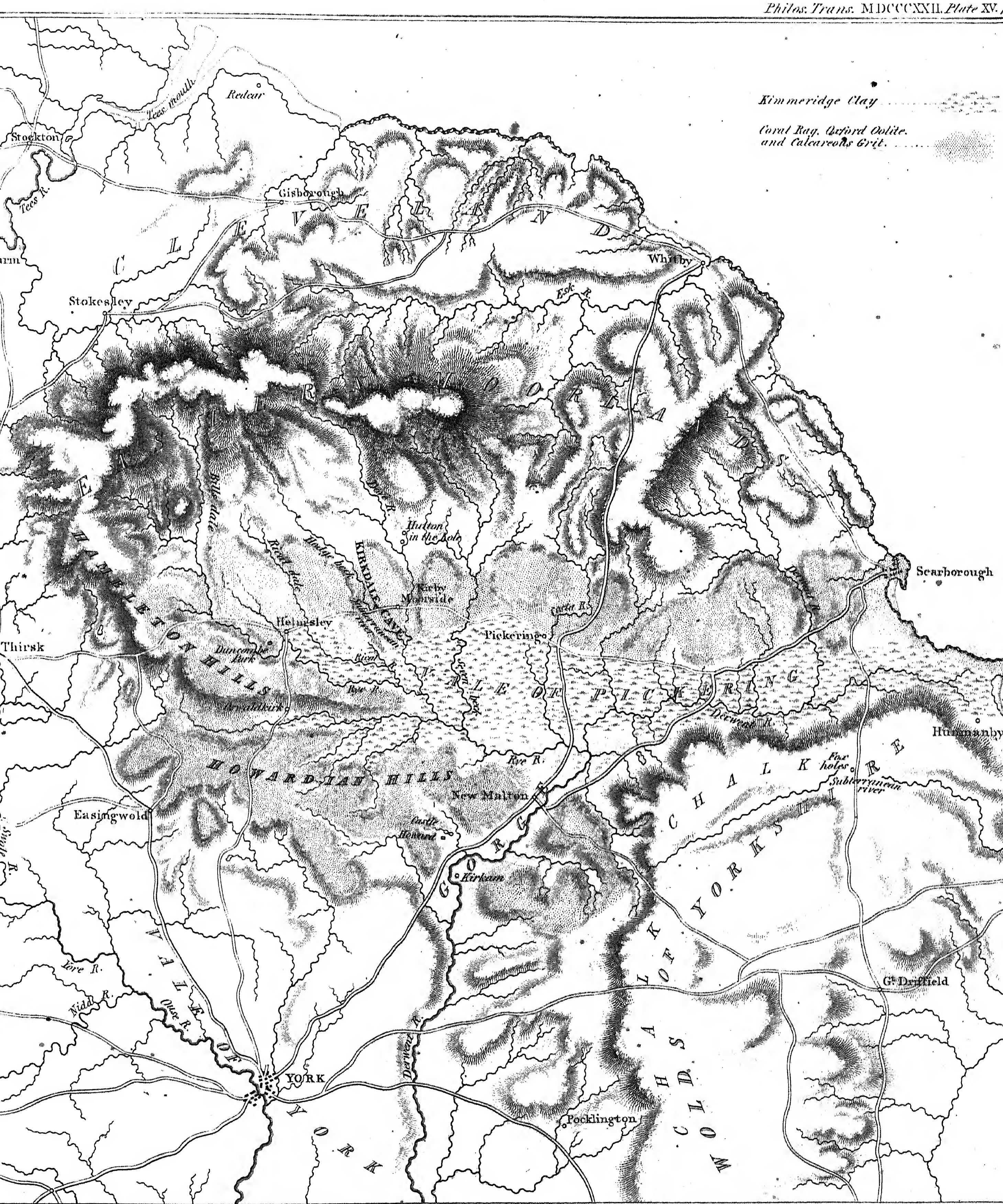




Autographic Curve of the Variations of the Barometer at Tottenham, in the Months of November and December, 1821.

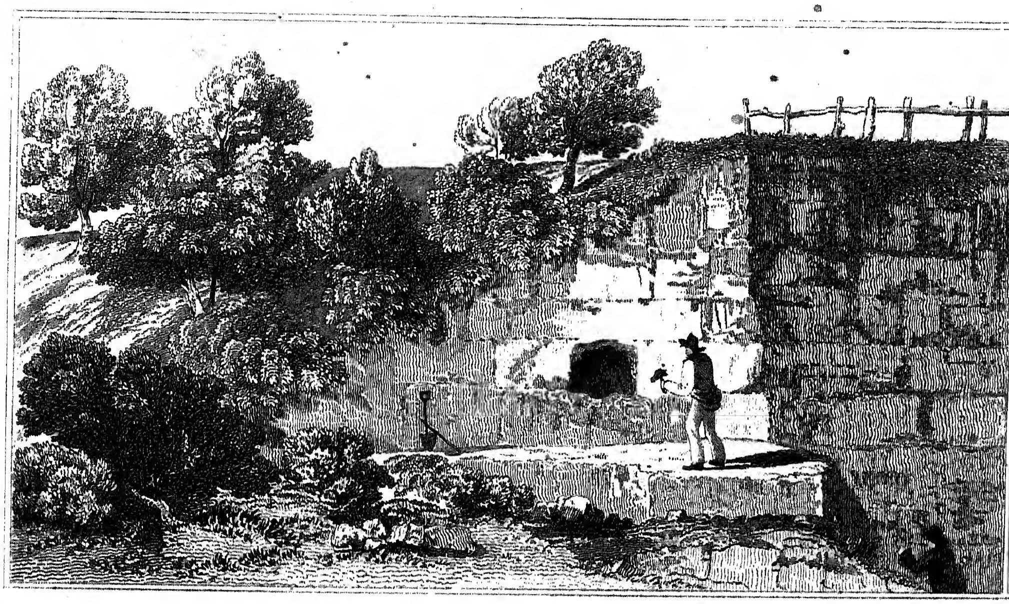





Kimmeridge Clay
Coral Rag, Oxford Oolite, and Calcareous Grit.
Redcar
Tees mouth
Stockton
Tees R.
Gisborough
CLEVELAND
Whitby
Stokesley
Esk R.
Dove R.
Hutton in the hole
KIRKDALE CAVE
Kirby Moorside
Helmsley
Thirsk
Duncombe Park
HAMBLETON HILLS
Rye R.
Pickering
VALE OF PICKERING
Scarborough
Hunmanby
Derwent R.
HOWARDIAN HILLS
New Malton
Castle Howard
Kirkham
GORGE
Easingwold
Four holes
Subterranean river
CHALK OF YORKSHIRE
Gt Driffield
Ure R.
Nidd R.
Ouse R.
VALE OF YORK
YORK
Derwent R.
Pocklington
WOLDS

J. Basire.

Fig. 1.



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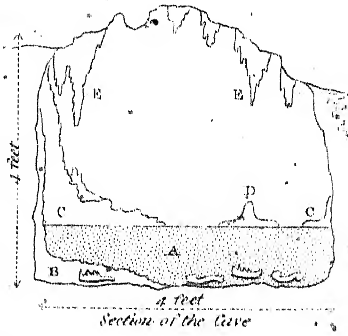
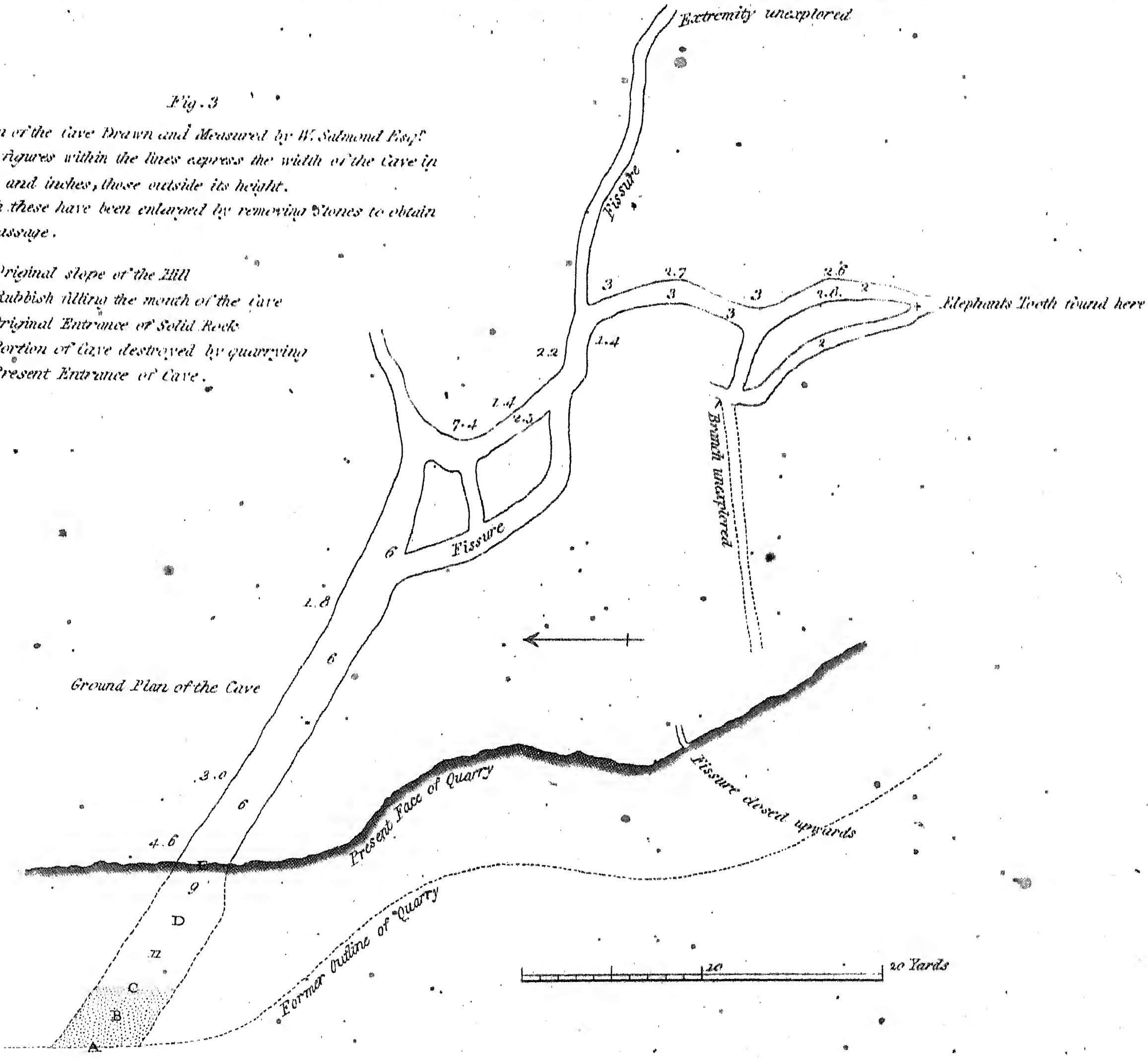


Section of the Cave

Fig. 3

Plan of the Cave Drawn and Measured by W. Salmond Esq.r
The figures within the lines express the width of the Cave in feet and inches, those outside its height.
Both these have been enlarged by removing Stones to obtain a Passage.

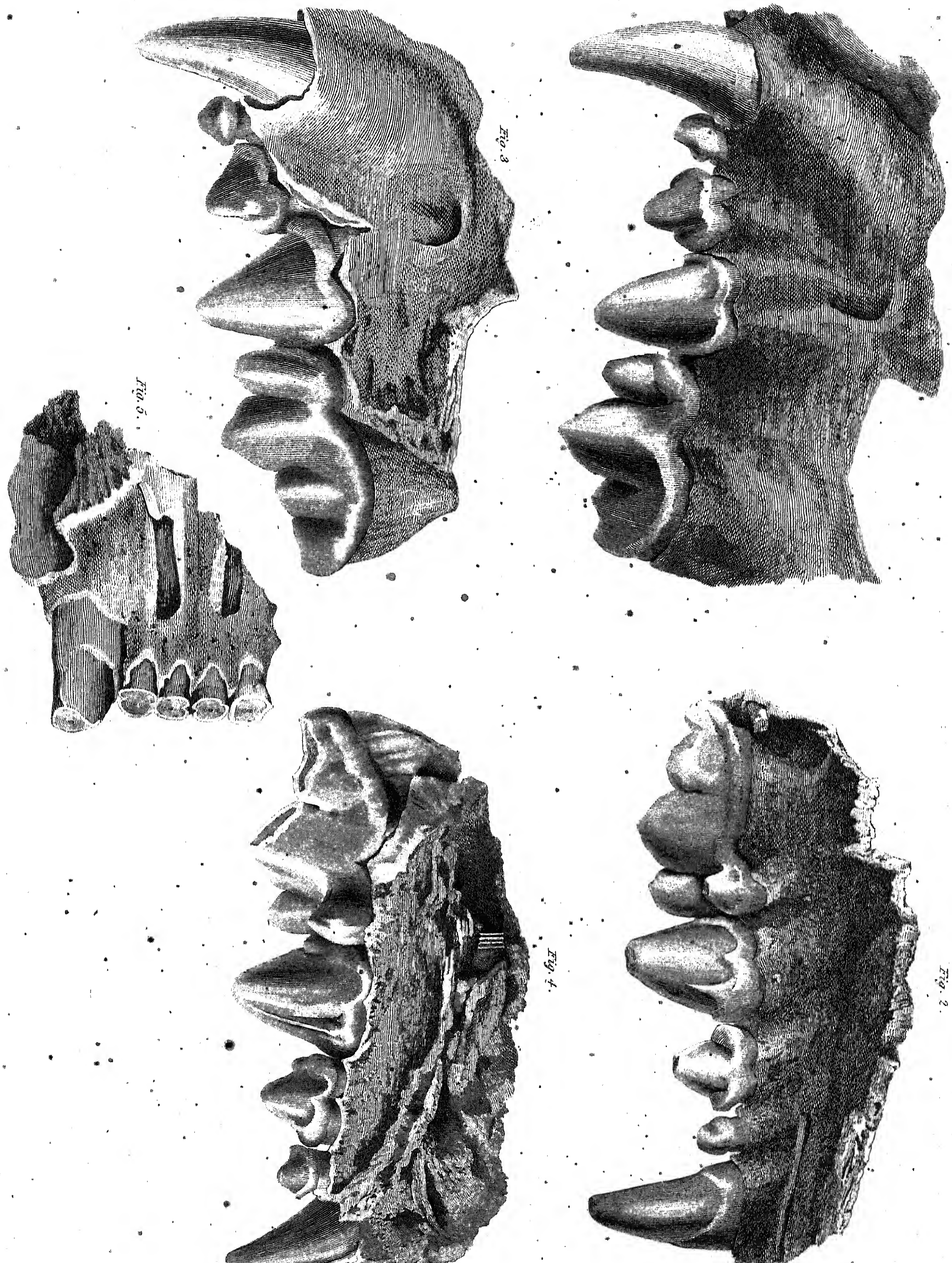
A. Original slope of the Hill
B. Rubbish filling the mouth of the Cave
C. Original Entrance of Solid Rock
D. Portion of Cave destroyed by quarrying
E. Present Entrance of Cave.



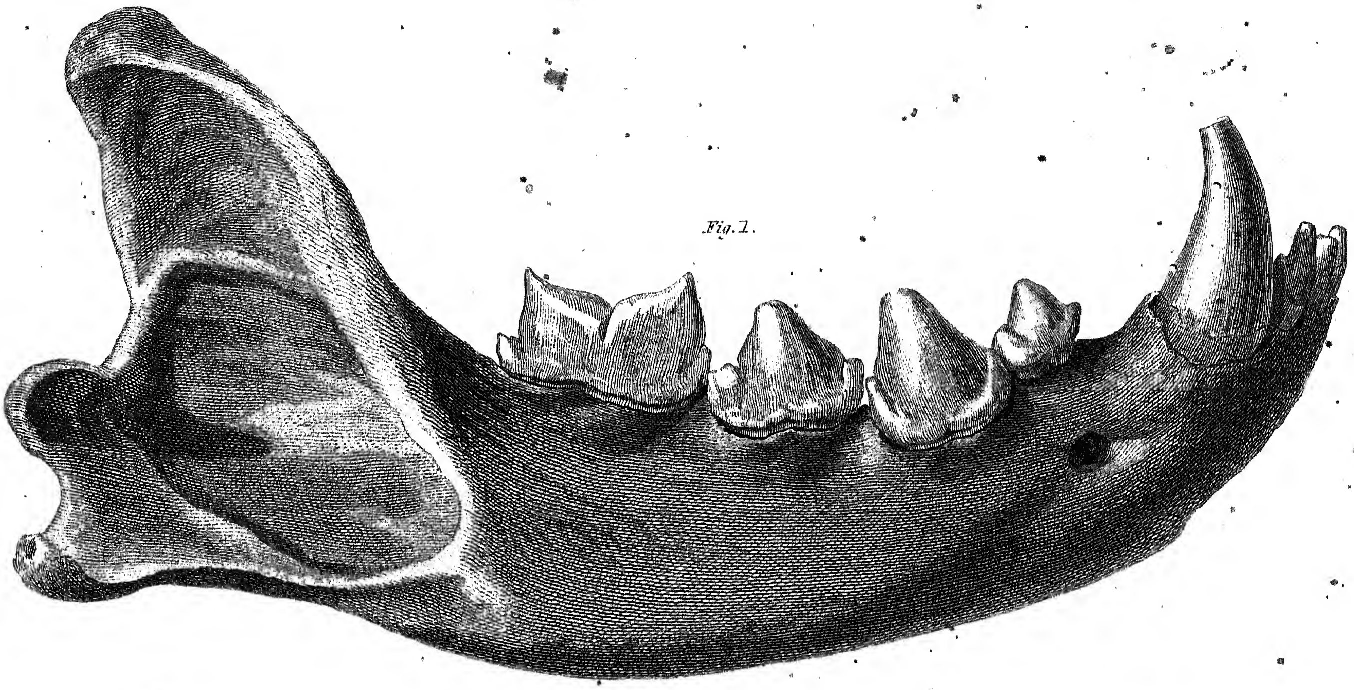
Ground Plan of the Cave

W. Buckland
T. Webster } del.

J.s Basire sc.



T. Webster del. Jas. Basire sc.



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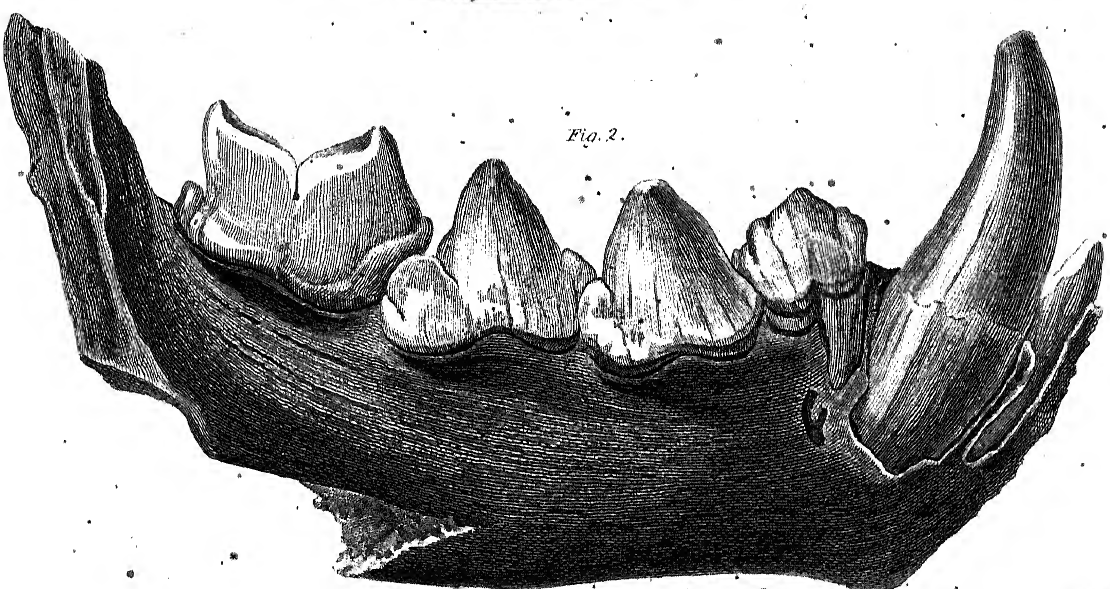

Fig. 2.

Fig. 3

T. Webster
W. Clift &
M. Morland } del.

Js Basire sc.

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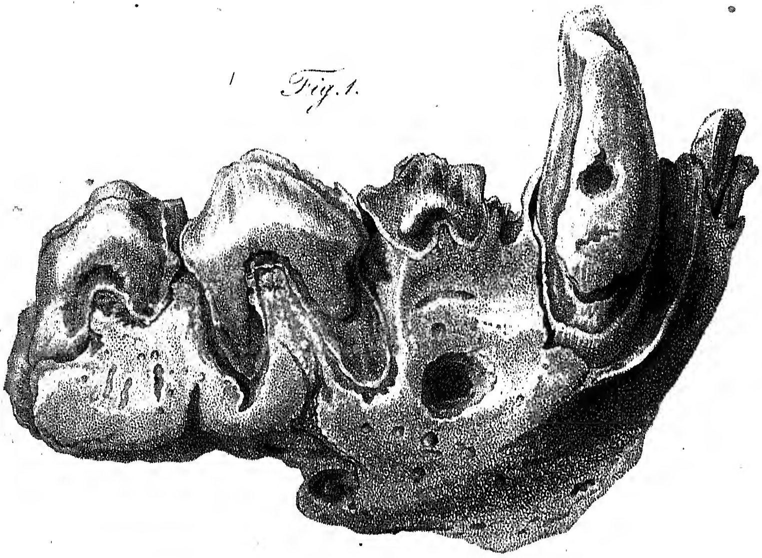

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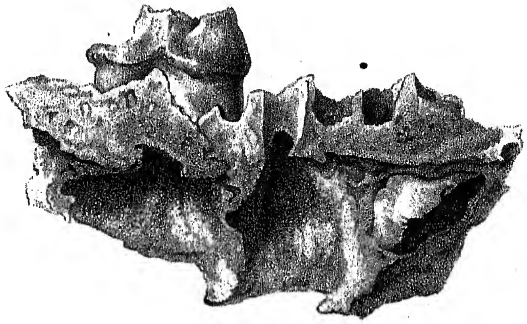

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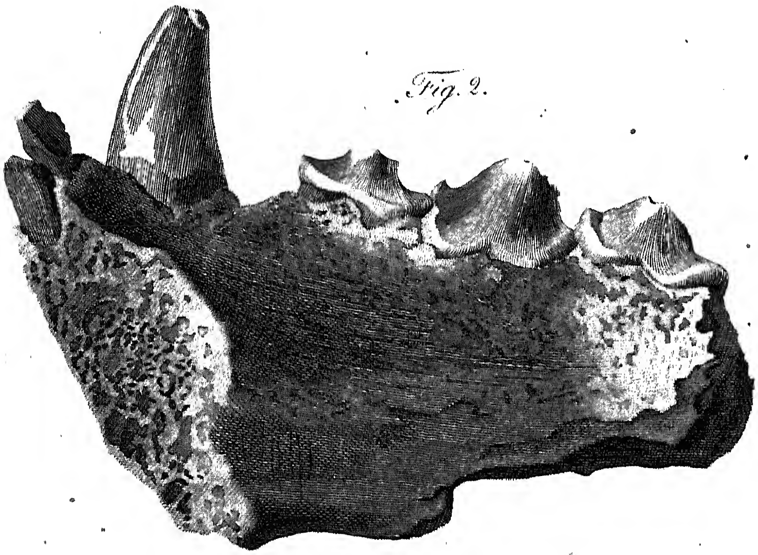

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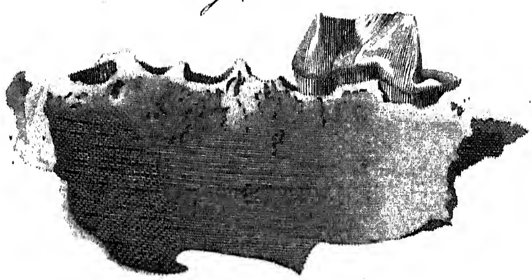

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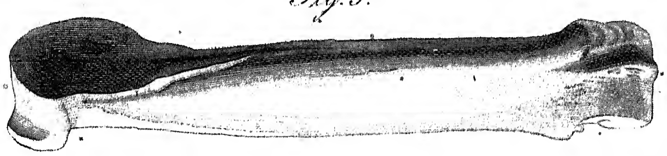


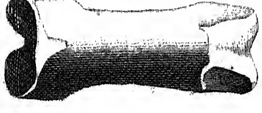
Fig. 6.

Fig. 7.

Fig. 8.

Fig. 9.

Fig. 10.



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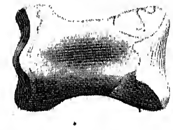
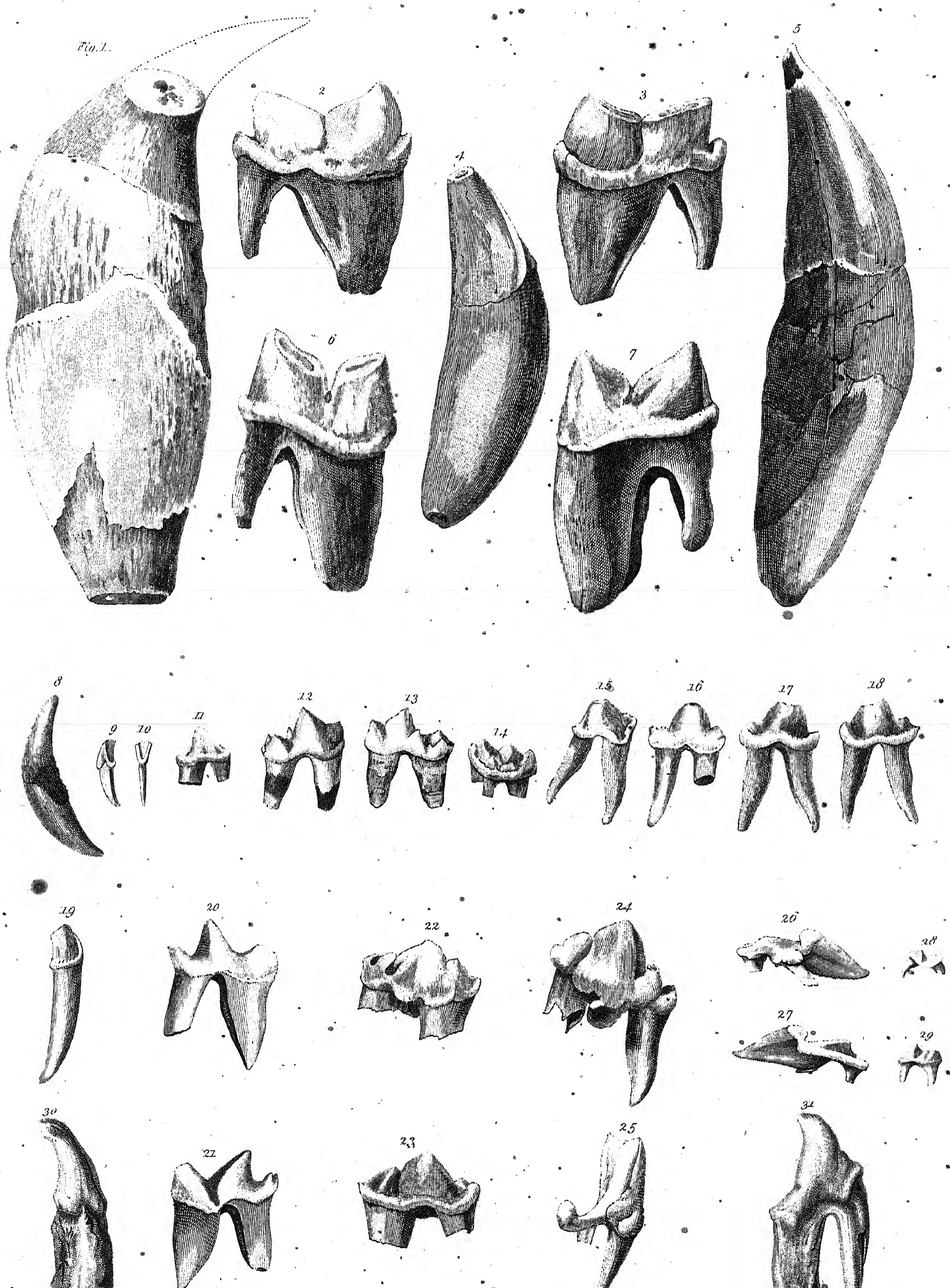


Fig. 12.

F. Duncombe
T. Webster } del.

J. Basire sc.



M. Morland
W. Clift & } del.t

J.s Basire sc.

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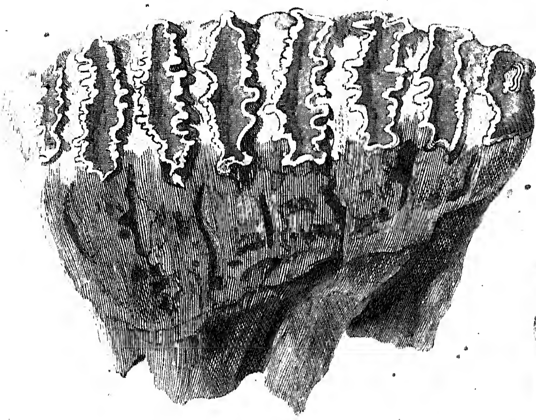

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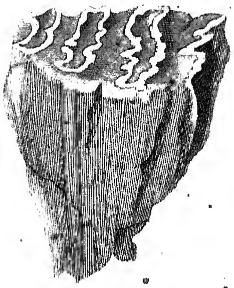

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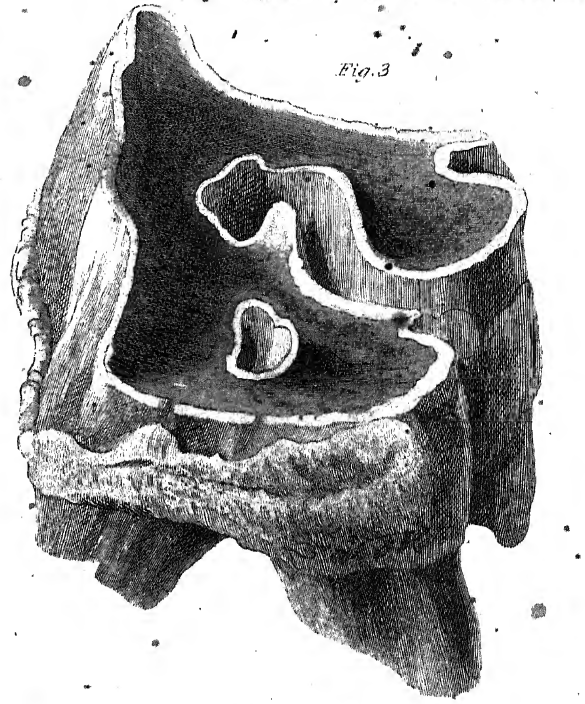

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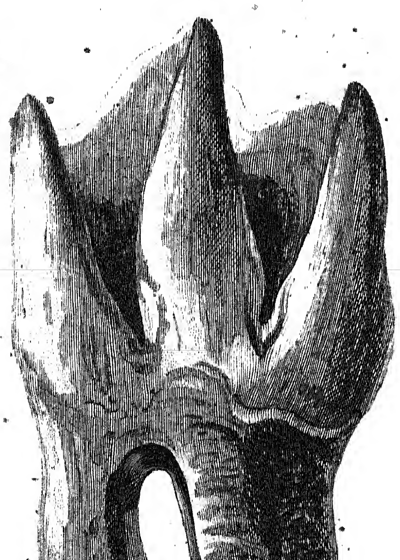

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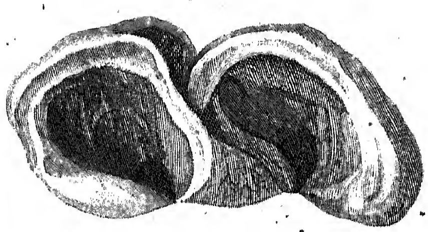

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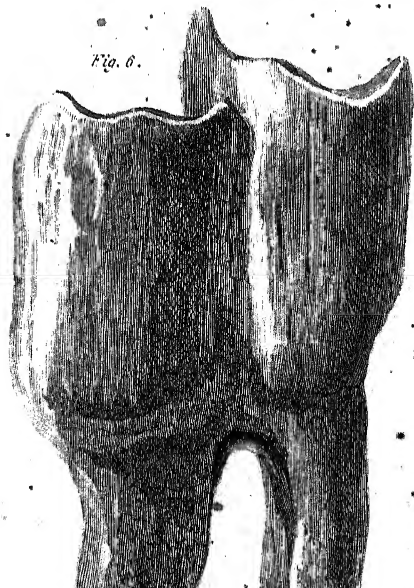

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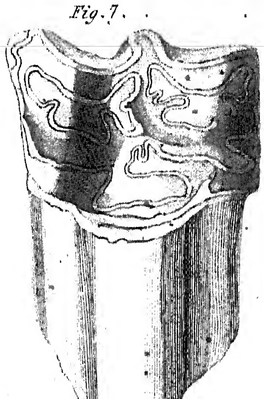

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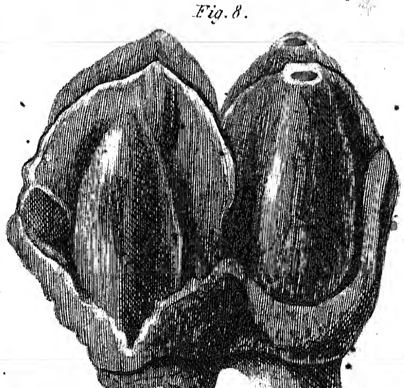

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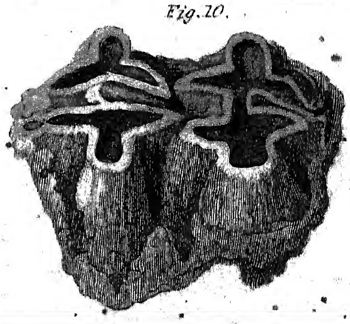
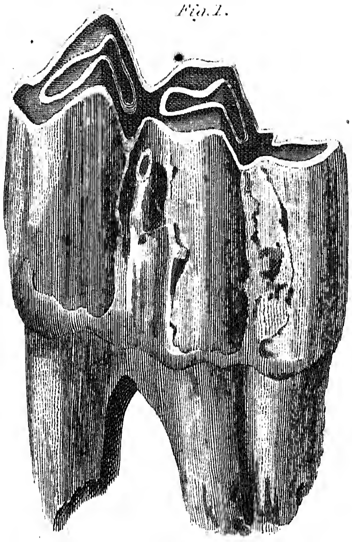


M. Morland
W. Clift &
T. Webster } del.

J. Basire sc.

Fig. 1.



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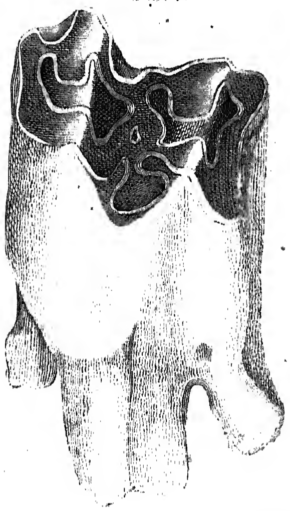

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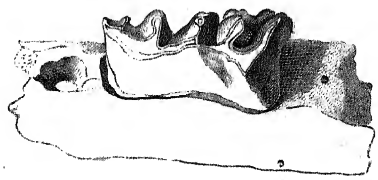

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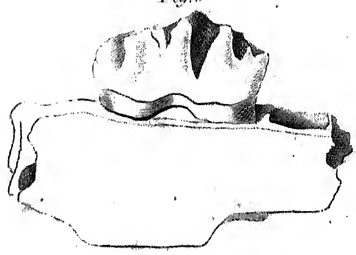
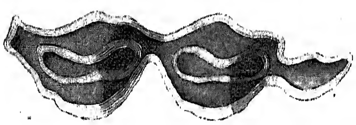


Fig. 2.



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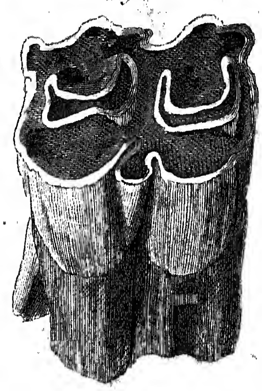


Fig. 8.

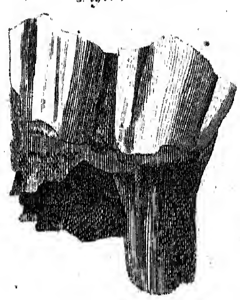


Fig. 3.

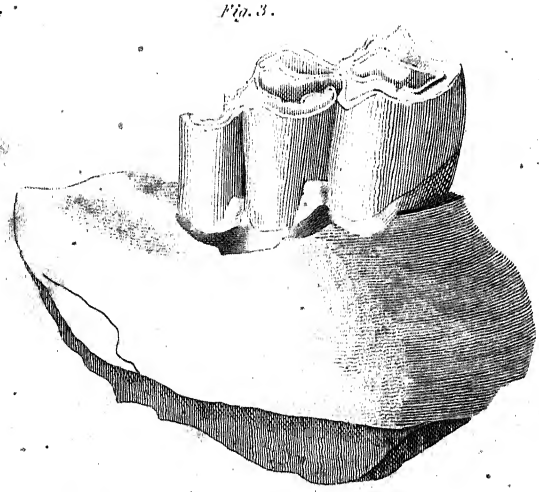


Fig. 9.

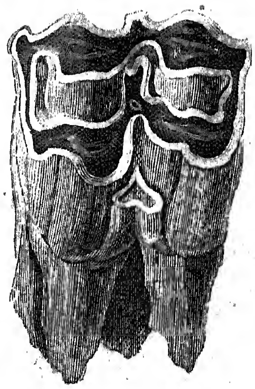


Fig. 10.

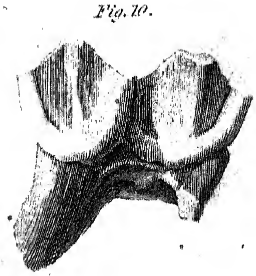


Fig. 13.

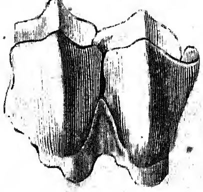


Fig. 14.

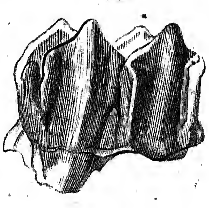
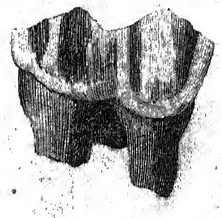


Fig. 11.

Fig. 12.



M. Morland.
F. Duncombe, } del.t
T. Webster.

J.s Basire sc.

Fig. 1.

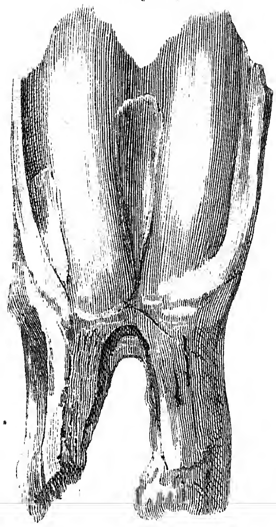


Fig. 3.

Fig. 2.

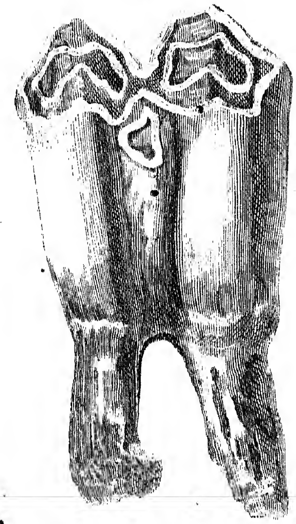
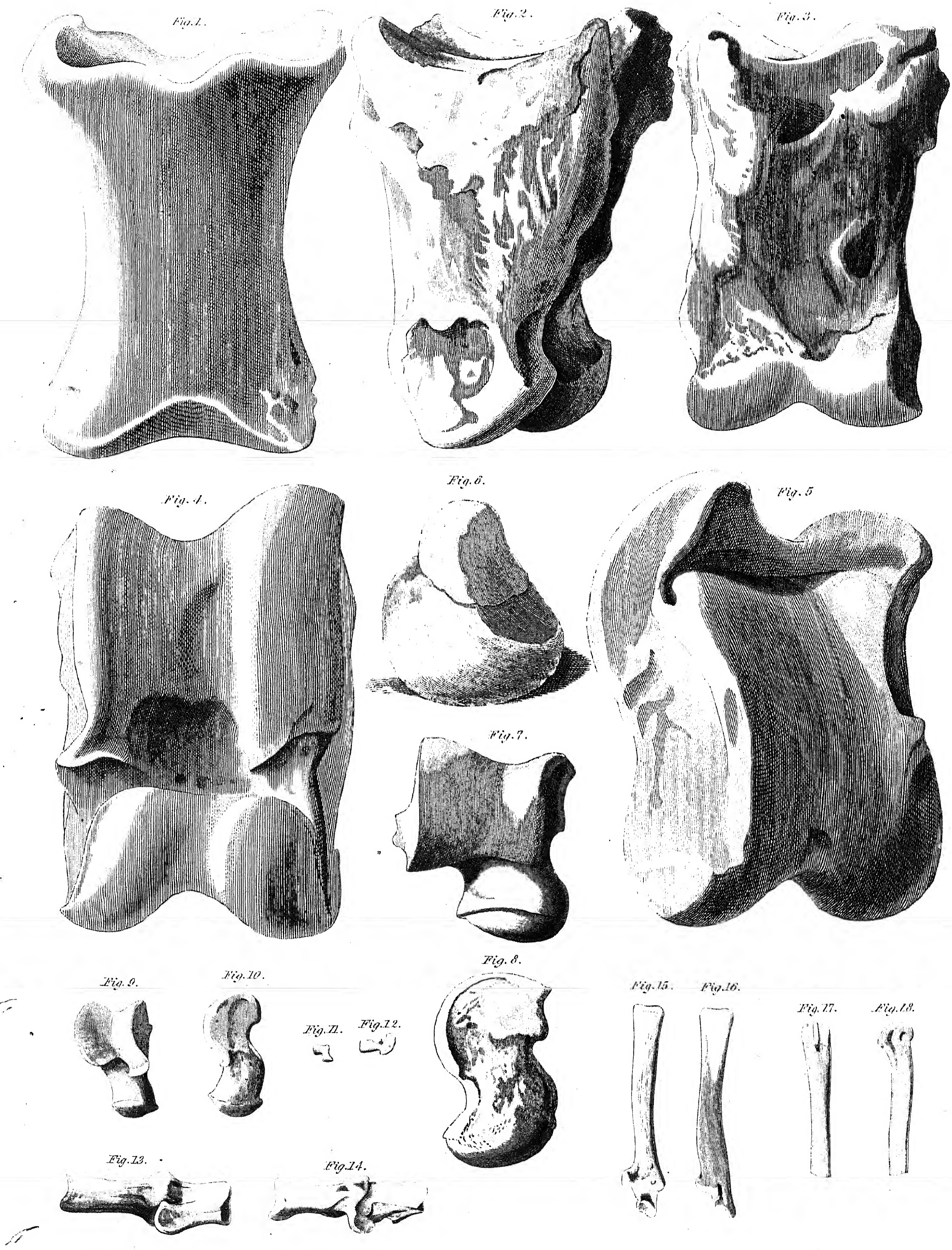


Fig. 5.

Fig. 4.

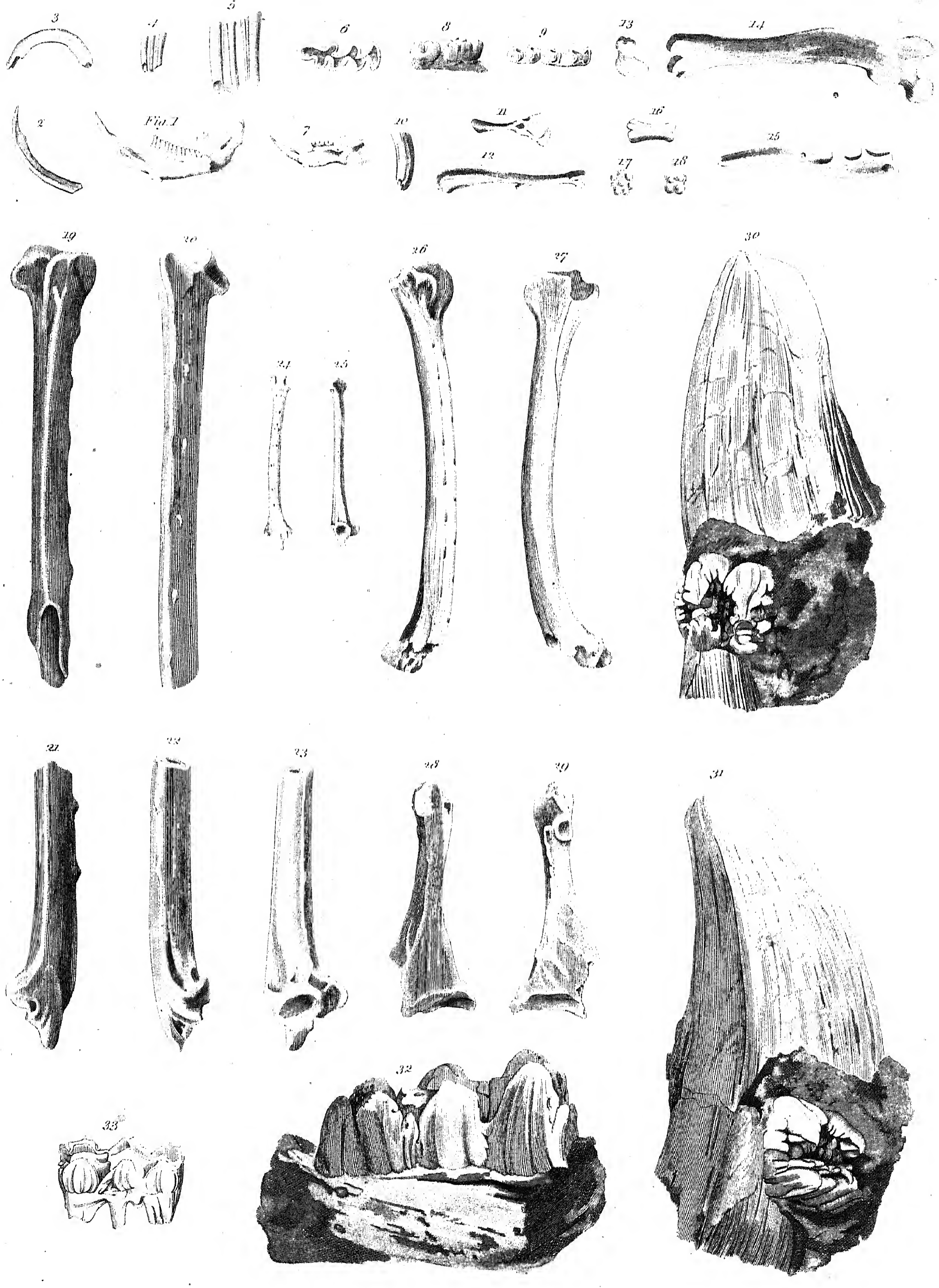
F. Duncombe del.

Js. Basire sc.



T. Webster del.

Js. Basire sc.

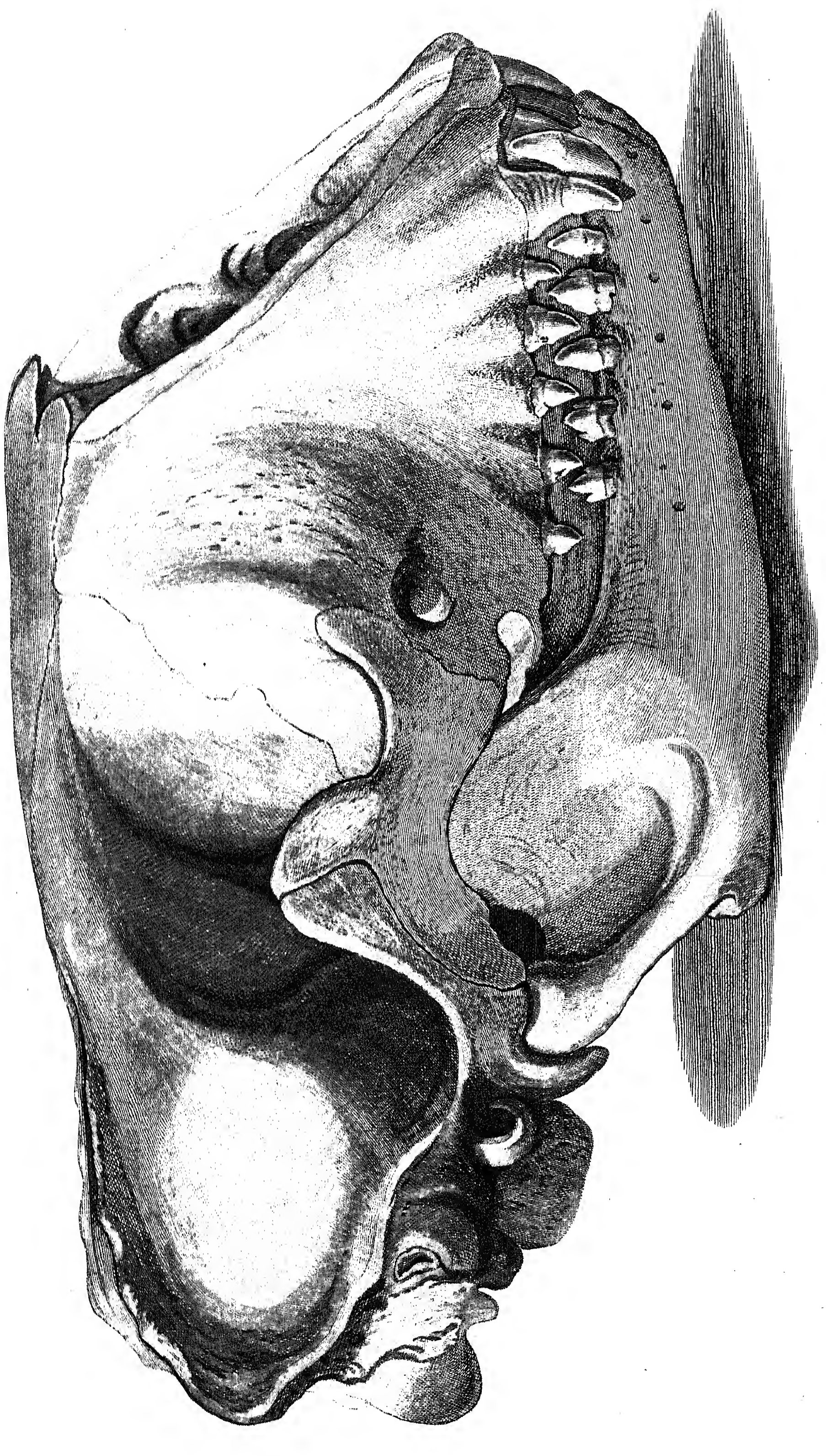


M. Morland,
T. Webster, } del.t
H. O'Neil,

J.s Basire sc.

W. Buckland. } del.
T. Webster }

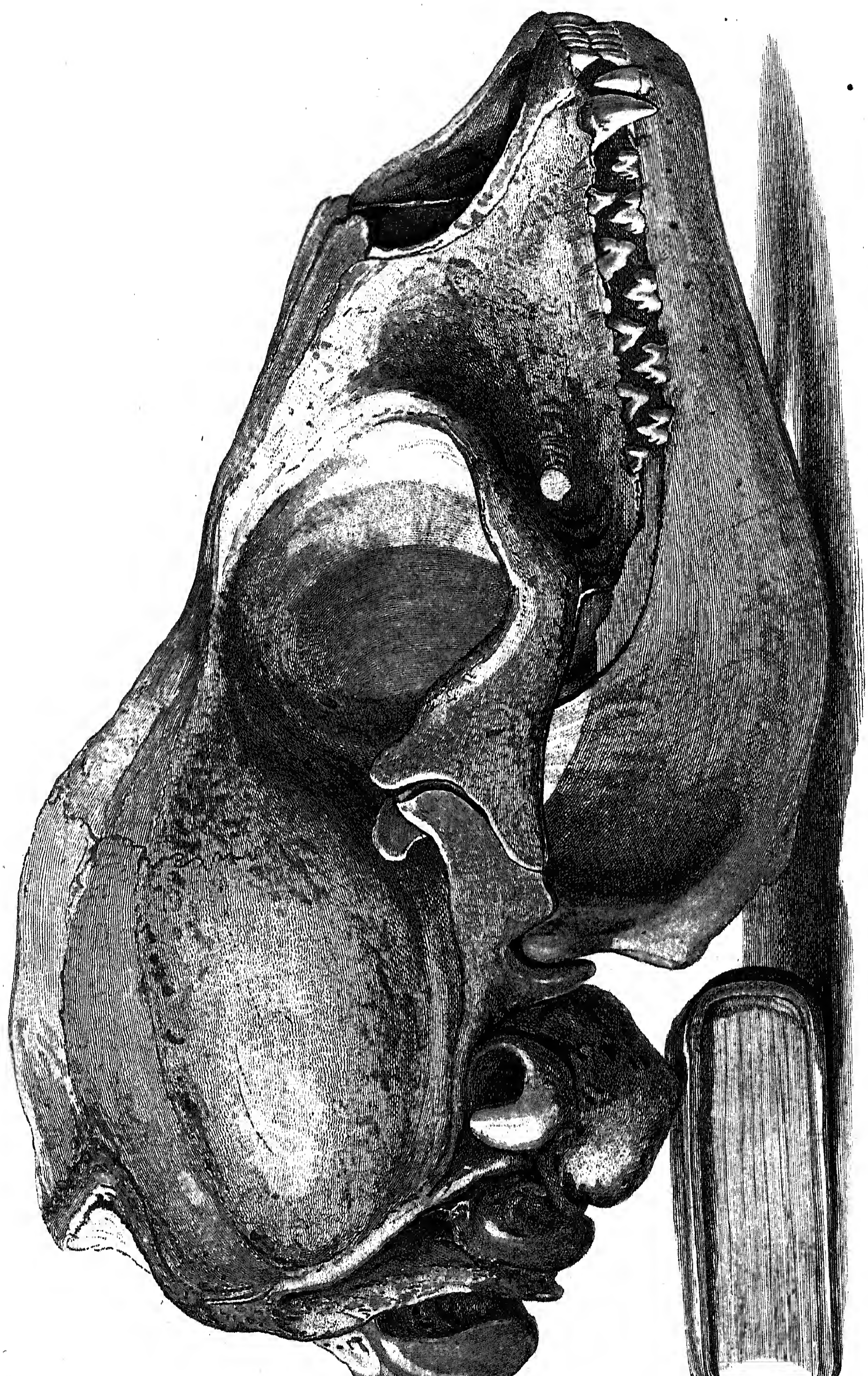
J. Basire sc.



Scale, eight inches to a foot.

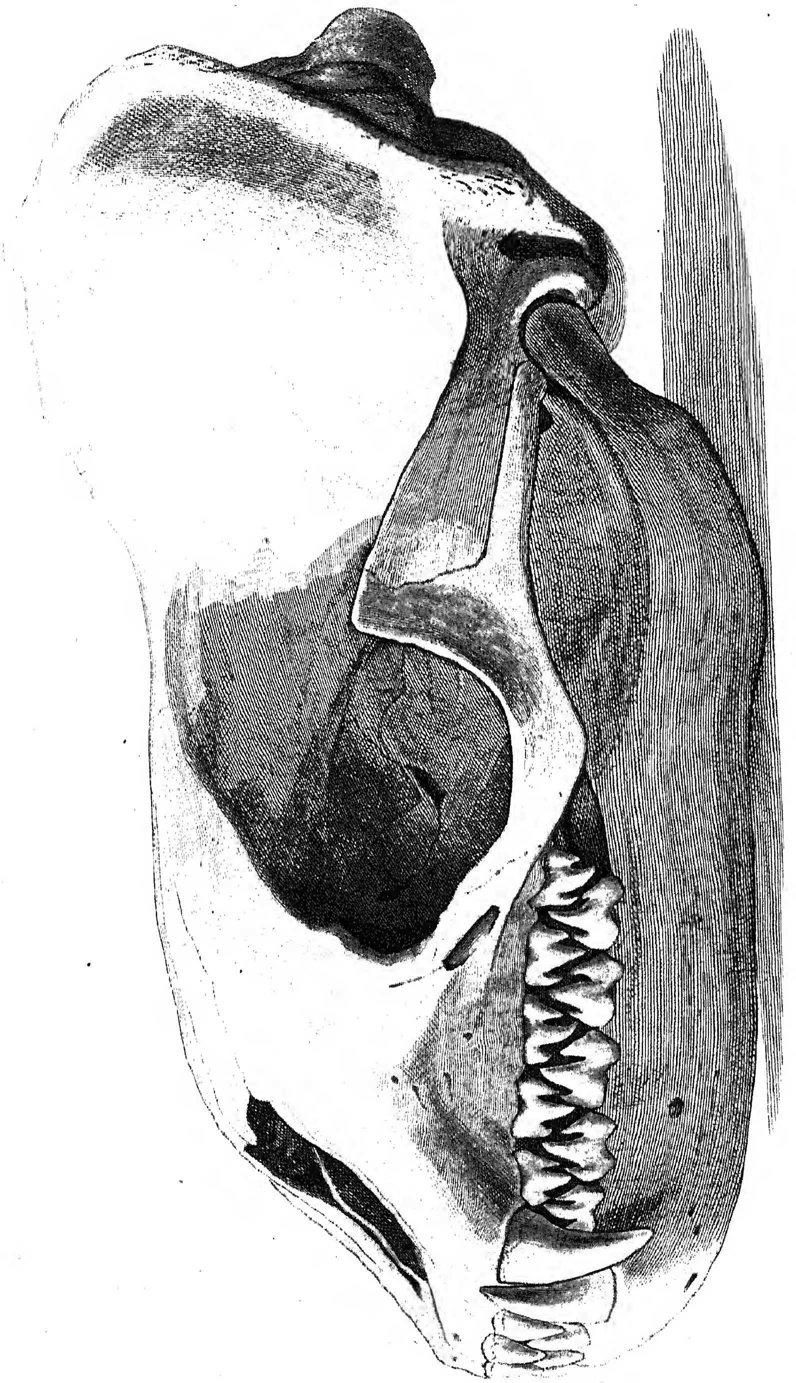
Wm Clift del.

Js Basire sc.



Natural size

W^m Clift del. Js Basire sc.



Scale half an inch to an inch.

Wm Clift del.

W. Basire sc.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXII.

PART II.

LONDON:

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MDCCCXXII.

CONTENTS.

CONTENTS.

PHILOSOPHICAL

TRANSACTIONS.

XIX. *Experiments and observations on the developement of magnetical properties in steel and iron by percussion.* *By* WILLIAM SCORESBY, *Jun. Esq. Communicated by Sir* HUMPHRY DAVY, *Bart. P. R. S.*

Read March 7th, 1822.

DR. GILBERT, so early as the year 1600, discovered that iron became sensibly magnetic on being hammered and drawn out while lying in a north and south direction; but I am not aware that either Dr. GILBERT, or any other person, ever gave to iron or steel, by this process, any considerable lifting power. I cannot indeed discover that any magnetical effect, by hammering, has been produced, beyond that of occasioning a deviation in the compass needle, or of giving to floating bars, or needles, the power of conforming their position to that of the magnetic meridian.

In the course of some experiments made in the autumn of 1820, I succeeded in determining, in a great measure, the principal laws by which the developement and destruction of magnetism in iron, by percussion, scowering, filing, bending, &c. are governed. As the result of this investigation is al-

I. *Experiments with a cylindrical bar of soft steel, for determining the effect of percussion, when the bar was held in a vertical position and resting upon a piece of metal not ferruginous. Length of the bar 6½ inches; diameter a quarter of an inch; weight 592 grains.*

Number of Blows.		Weight lifted.	Distance of Compass and Bar.	Deviation.	Hammer.
at each trial.	Total.				
1	1	2	3	8	II.
1	2	—	....	10	....
5	7	4	....	12	....
10	17	6½	....	12½	....
5	22	6½	....	12½	I.

This bar was next hammered upon a large mass of free-stone: after twenty blows with the large hammer had been struck upon it, the deviation it was found had not increased, but still remained at 12½°.

Dr. GILBERT tried this experiment on iron; but instead of hammering it in the direction of the dipping needle, or in a vertical direction, which produces almost an equal effect, he placed it horizontally in the magnetic meridian, which, in London, is a plane elevated only 19½ degrees above the magnetic equator. Now, as my former experiments proved that hammering iron in the plane of the magnetic equator destroys its polarity, it is evident, that a very small part only of the full influence of percussion must have been obtained by Dr. GILBERT.

As magnetism in steel is more readily developed by the contact of magnetisable substances, and particularly if these substances be already magnetic, it occurred to me, that the magnetising effects of percussion might be greatly increased

by hammering the steel bar, with its lower end resting on the upper end of a large rod of iron or soft steel, both the masses being held in a vertical position: and that if the rod were first rendered magnetic by hammering, the effect on the bar would probably be augmented. The following experiments prove that these opinions were not incorrect.

II. *Experiments for determining the effect of percussion on a soft steel bar, when the bar was held in a vertical position and resting upon a parlour poker.*

[*a. Both the bar and the poker were first deprived of magnetism.*]

Number of Blows.		Weight lifted.	Distance of Compass and Bar.	Deviation.	No. of the Hammer.
at each trial.	Total.				
		Grains.	Inches.	°	
1	1	$6\frac{1}{2}$	3	13	II.
1	2	14	. . .	16	. . .
1	3	. . .	. . .	18	. . .
4	7	37	. . .	21	. . .
5	12	45	. . .	25	. . .
10	22	88	. . .	27	. . .
20	42	88	. . .	30	. . .
30	72	. . .	. . .	31	. . .
10	82	. . .	. . .	$31\frac{1}{2}$	. . .

The effect seemed now to be at a maximum, for more blows with the same hammer produced no alteration; but on substituting a larger hammer, the deviation was augmented.

[*b. Change of Hammer.*]

3	85	. . .	. . .	33	I.
5	90	130	. . .	34	
3	93	. . .	. . .	30!	

The bar was now inverted, so that the north pole was upward.

[*c. Change of the ends of the Bar.*]

1	. . .	0	3	5	II.
1	2	0	. . .	2	Poles changed.

In this stage of the experiments the poker had become strongly magnetic, and its magnetism was farther increased by hammering it upon another bar of iron.

[*d.*. *The bar deprived of magnetism; but the poker rendered magnetic by hammering.*]

Number of Blows. at each trial.	Number of Blows. Total.	Weight lifted.	Distance of Compass and Bar.	Deviation.	Hammer.
			Inches.	°	
1	1	. . .	3	16	I.
[The experiment repeated.]					
1	1	. . .	3	18	I.
1	1	. . .	3	17	II.
1	1	. . .	. . .	12½	III.
100	101	. . .	. . .	27	. . .
100	201	. . .	. . .	28½	. . .
2	203	. . .	. . .	28½	I.

The bruised end of the bar was now hammered horizontally until it formed a kind of cup; this being easily compressed by the hammer, the effect was increased.

[*e. Bar deprived of magnetism; poker hammered.*]

1	1	. . .	3	23	I.
1	1	. . .	3	22	II.
5	6	. . .	. . .	30	. . .
4	10	188	. . .	36 ?	I.
5	15	188	. . .	33	. . .

III. *For determining the effect of percussion on magnets properly tempered, a flat bar magnet of a blue temper was employed; it was $7\frac{7}{10}$ inches long, $\frac{1}{2}$ an inch broad, and $\frac{1}{7}$th thick, and weighed 1170 grains.*

[*a. The magnet held vertically on a rod of iron; South end upward.*

No. of Blows. at each trial.	No. of Blows. Total.	End of the Magnet upward.	Distance of the Compass.	Deviation.
			Inches.	°
0	0	. . .	8	45
5	5	S.	. . .	37
5	10	. . .	. . .	32
10	20	. . .	. . .	30
10	30	. . .	. . .	28
10	40	. . .	. . .	27
20	60	. . .	. . .	26
20	80	. . .	. . .	25
20	100	. . .	. . .	25
	[*b.* North end up.]			
0	0	. . .	8	25
1	1	N.	. . .	20
10	11	. . .	. . .	16
20	30	. . .	. . .	14

[*c. Bar again magnetised and hammered upon a piece of pewter.*]

0	0	. . .	8	50
10	10	S.	. . .	37
10	20	. . .	. . .	33
10	30	N.	. . .	27
10	40	. . .	. . .	26
20	60	. . .	. . .	24
20	80	. . .	. . .	24
20	100	S.	. . .	24
20	120	N.	. . .	23
20	140	. . .	. . .	23
20	160	S.	. . .	$23\frac{1}{2}$

steel bar used in the first and second series of experiments, and weighing about 600 grains, was hammered for a considerable time while held vertically upon the poker. The greatest effect which I could produce with the large hammer, was a deflection of the compass needle, at the distance of three inches, of thirteen degrees; in this state it lifted a nail of $6\frac{1}{2}$ grains, but refused one of 11 grains weight.

6. A *cast iron* bar of the same size and form as the last, became capable of lifting a nail of 37 grains weight. After it had acquired this power, its magnetism was nearly destroyed by five blows with the north pole upward.

The strong magnetising effect of percussion on soft steel, induced me to apply the property to the formation of magnets. For this purpose I procured two bars of soft steel, 30 inches long and an inch broad; also six other flat bars of soft steel 8 inches long and half an inch broad, and a large bar of soft iron. The large steel and iron bars were not however absolutely necessary, as common pokers answer the purpose very well; but I was desirous to accelerate the process by the use of substances capable of aiding the developement of the magnetical properties in steel. The large iron bar was first hammered in a vertical position. It was then laid on the ground with its acquired south pole towards the south, and upon this end of it the large steel bars were rested while they were hammered; they were also hammered upon each other. On the summit of one of the large steel bars, each of the small bars, held also vertically, was

hammered in succession, and in a few minutes they had all acquired considerable lifting powers. Two of the smaller bars, connected by two short pieces of soft iron in the form of a parallelogram, were now rubbed with the other four bars in the manner of CANTON. These were then changed for two others: and these again for the last two. After treating each pair of bars in this way for a number of times, and changing them whenever the manipulations had been continued for about a minute, the whole of the bars were at length found to be magnetised to saturation; each pair readily lifting above eight ounces!

In accomplishing this object, I took particular care that no magnetic substance was used in the process. All the bars were freed of magnetism before the experiment, so that none of them, not even the largest, produced a deviation of five degrees on the compass at three inches distance. The hammers were also carefully examined. Any bars which had been strongly magnetised, and had had their magnetisms destroyed or neutralised (either by hammering, heating, or by the simultaneous contact of the two poles of another magnet placed transversely,) I always found had a much greater facility for receiving polarity in the same direction as before, than the contrary. Hence it generally happened, that one blow with the original north end downward, produced as much effect as two or three blows did with the original south end downward. I also observed that the polarity of *pokers*, generally supposed to be permanent and considerable in intensity, was rather transient and weak: for in no instance did I meet with a poker, the magnetism of which I could not

destroy by a blow or two with a hammer, on the point; and, in general, two blows, even when the poker was held in the hand, and not rested upon any thing, were sufficient to invert the poles.

Liverpool,
18*th February,* 1822.

XX. *On the Alloys of steel. By* J. Stodart, *Esq. F.R.S. and Mr.* M. Faraday, *Chemical Assistant in the Royal Institution. Communicated by* J. Stodart, *Esq. F. R. S.*

Read March 21, 1822.

The alloys of steel made on a small scale in the laboratory of the Royal Institution proving to be good, and the experiments having excited a very considerable degree of interest both at home and abroad, gave encouragement to attempt the work on a more extended scale, and we have now the pleasure of stating, that alloys similar to those made in the Royal Institution, have been made for the purpose of manufacture; and that they prove to be, in point of excellence, in every respect equal, if not superior, to the smaller productions of the laboratory. Previous however, to extending the work, the former experiments were carefully repeated, and to the results were added some new combinations, namely, steel with palladium, steel with iridium, and osmium, and latterly, steel with chromium. In this last series of experiments we were particularly fortunate, having by practice acquired considerable address in the management of the furnaces, and succeeded in procuring the best fuel for the purpose. Notwithstanding the many advantages met with in the laboratory of the Royal Institution, the experiments were frequently rendered tedious from causes often unexpected, and sometimes difficult to overcome; among these, the failure of crucibles was perhaps the most perplexing. We have

never yet found a crucible capable of bearing the high degree of temperature required to produce the perfect reduction of titanium; indeed we are rather disposed to question whether this metal has ever been so reduced: our furnaces are equal* (if any are) to produce this effect, but hitherto we have failed in procuring a crucible.

The metals that form the most valuable alloys with steel are silver, platina, rhodium, iridium and osmium, and palladium; all of these have now been made in the large way, except indeed the last named. Palladium has, for very obvious reasons, been used but sparingly; four pounds of steel with $\frac{1}{100}$ part of palladium, has however been fused at once, and the compound is truly valuable, more especially for making instruments that require perfect smoothness of edge.

We are happy to acknowledge the obligations due from us to Dr. WOLLASTON, whose assistance we experienced in every stage of our progress, and by whom we were furnished with all the scarce and valuable metals; and that with a liberality which enabled us to transfer our operations from the laboratory of the chemist, to the furnace of the maker of cast steel.

In making the alloys on a large scale, we were under the necessity of removing our operations from London to a steel furnace at Sheffield; and being prevented by other avocations from giving personal attendance, the superintendence of the work was consequently intrusted to an intelligent and confidential agent. To him the steel, together with the alloying metals in the exact proportion, and in the most favourable state for the purpose, was forwarded, with instructions to see

* We have succeeded in fusing in these furnaces rhodium, and also, though imperfectly, platinum in crucibles.

the whole of the metals, and nothing else, packed into the crucible, and placed in the furnace, to attend to it while there, and to suffer it to remain for some considerable time in a state of thin fusion, previous to its being poured out into the mould. The cast ingot was next, under the same superintendence, taken to the tilting mill, where it was forged into bars of a convenient size, at a temperature not higher than just to render the metal sufficiently malleable under the tilt hammer. When returned to us, it was subjected to examination both mechanical and chemical, as well as compared with the similar products of the laboratory From the external appearance, as well as from the texture of the part when broken by the blow of the hammer, we were able to form a tolerably correct judgment as to its general merits; the hardness, toughness, and other properties, were farther proved by severe trials, after being fashioned into some instrument, or tool, and properly hardened and tempered.

It would prove tedious to enter into a detail of experiments made in the Royal Institution; a brief notice of them will at present be sufficient. After making imitations of various specimens of meteoric iron by fusing together pure iron and nickel, in proportions of 3 to 10 per cent, we attempted making an alloy of steel with silver, but failed, owing to a superabundance of the latter metal; it was found, after very many trials, that only the $\frac{1}{500}$ part of silver would combine with steel, and when more was used a part of the silver was found in the form of metallic dew lining the top and sides of the crucible; the fused button itself was a mere mechanical mixture of the two metals, globules of silver being pressed out of the mass by contraction in cooling, and more of these globules being forced out by the hammer in forging; and far-

ther, when the forged piece was examined, by dissecting it with diluted sulphuric acid, threads or fibres of silver were seen mixed with the steel, having something of the appearance of steel and platina when united by welding: but when the proportion of silver was only $\frac{1}{500}$ part, neither dew, globules, nor fibres appeared, the metals being in a state of perfect chemical combination, and the silver could only be detected by a delicate chemical test.

With platina and rhodium, steel combines in every proportion; and this appears also to be the case with iridium and osmium: from 1 to 80 per cent. of platina was perfectly combined with steel, in buttons of from 500 to 2000 grains. With rhodium, from 1 to 50 per cent. was successfully used. Equal parts by weight of steel and rhodium gave a button, which, when polished, exhibited a surface of the most exquisite beauty: the colour of this specimen is the finest imaginable for a metallic mirror, nor does it tarnish by long exposure to the atmosphere: the specific gravity of this beautiful compound is 9.176. The same proportion of steel and platina gave a good button, but a surface highly crystalline renders it altogether unfit for a mirror. In the laboratory we ascertained that, with the exception of silver, the best proportion of the alloying metal, when the object in view was the improvement of edge tools, was about $\frac{1}{100}$ part, and in this proportion they have been used in the large way. It may be right to notice, that in fusing the metals in the laboratory no flux whatever was used, nor did the use of any ever appear to be required.

Silver being comparatively of little value with some of the alloying metals, we were disposed to make trial with it as the first experiment in the large way. 8lbs. of very good

Indian steel was sent to our agent, and with it $\frac{1}{500}$ part of pure silver: a part of this was lost owing to a defect in the mould; a sufficient quantity was however saved, to satisfy us as to the success of the experiment. This, when returned, had the most favourable appearance both as to surface and fracture; it was harder than the best cast steel, or even than the Indian wootz, with no disposition whatever to crack, either under the hammer, or in hardening. Some articles, for various uses, have been made from this alloy; they prove to be of a very superior quality; its application will probably be extended not only to the manufacture of cutlery, but also to various descriptions of tools; the trifling addition of price cannot operate against its very general introduction. The silver alloy may be advantageously used for almost every purpose for which good steel is required.

Our next experiment made in the large way, was with steel and platina. 10lbs. of the same steel, with $\frac{1}{100}$ part of platina, the latter in the state produced by heating the ammonia muriate in a crucible to redness, was forwarded to our agent, with instructions to treat this in the same way as the last named metals. The whole of this was returned in bars remarkable for smoothness of surface and beauty of fracture. Our own observation, as well as that of the workmen employed to make from it various articles of cutlery, was, that this alloy, though not so hard as the former, had considerably more toughness: this property will render it valuable for every purpose where tenacity, as well as hardness, is required; neither will the expense of platina exclude it from a pretty general application in the arts; its excellence will much more than repay the extra cost.

The alloys of steel with rhodium have also been made in the large way, and are perhaps the most valuable of all; but these, however desirable, can never, owing to the scarcity of the metal, be brought into very general use. The compound of steel, iridium and osmium, made in the large way, is also of great value; but the same cause, namely, the scarcity and difficulty of procuring the metals, will operate against its very general introduction. A sufficient quantity of these metals may, perhaps, be obtained to combine with steel for the purpose of making some delicate instruments, and also as an article of luxury, when manufactured into razors. In the mean time, we have been enabled, repeatedly, to make all these alloys (that with palladium excepted) in masses of from 8 to 20lbs. each; with such liberality were we furnished with the metals from the source already named.

A point of great importance in experiments of this kind was, to ascertain whether the products obtained were exactly such as we wished to produce. For this purpose, a part of each product was analysed, and in some cases the quantity ascertained; but it was not considered necessary in every case to verify the quantity by analysis, because, in all the experiments made in the laboratory, the button produced after fusion was weighed, and if it fell short of the weight of both metals put into the crucible, it was rejected as imperfect, and put aside. When the button gave the weight, and on analysis gave proofs of containing the metal put in to form the alloy, and also on being forged into a bar and acted on by acids, presented an uniform surface, we considered the evidence of its composition as sufficiently satisfactory. The processes of analysis, though simple, we shall briefly state; the informa-

tion may be desirable to others who may be engaged on similar experiments; and farther, may enable every one to detect any attempt at imposition. It would be very desirable at present, to possess a test as simple, by which we could distinguish the wootz, or steel of India, from that of Europe; but this, unfortunately, requires a much more difficult process of analysis.

To ascertain if platina is in combination with steel, a small portion of the metal, or some filings taken from the bar, is to be put into dilute sulphuric acid; there will be rapid action; the iron will be dissolved, and a black sediment left, which will contain carbon, hydrogen, iron and platina; the carbon and hydrogen are to be burnt off, the small portion of iron separated by muriatic acid, and the residuum dissolved in a drop or two of nitro-muriatic acid. If a piece of glass be moistened with this solution, and then heated by a spirit lamp and the blow pipe, the platina is reduced, and forms a metallic coating on the glass.

In analysing the alloy of steel and silver, it is to be acted on by dilute sulphuric acid, and the powder boiled in the acid; the silver will remain in such a minute state of division, that it will require some time to deposit. The powder is then to be boiled in a small portion of strong muriatic acid;* this will dissolve the iron and silver, and the latter will fall down as a chloride of silver on dilution with water; or the powder may be dissolved in pure nitric acid, and tested by muriatic acid and ammonia.

* Although it is a generally received opinion that muriatic acid does not act on silver, yet that is not the case; pure muriatic acid dissolves a small portion of silver very readily.

The alloy of steel and palladium, acted on by dilute sulphuric acid, and boiled in that acid, left a powder which, when the charcoal was burnt from it, and the iron partly separated by cold muriatic acid, gave on solution in hot muriatic acid, or in nitro-muriatic acid, a muriate of palladium ; the solution, when precipitated by prussiate of mercury, gave prussiate of palladium ; and a glass plate moistened with it and heated to redness, became coated with metallic palladium.

The residuum of the rhodium alloy obtained by boiling in diluted sulphuric acid, had the combustible matter burnt off, and the powder digested in hot muriatic acid : this removed the iron ; and by long digestion in nitro-muriatic acid, a muriate of rhodium was formed, distinguishable by its colour, and by the triple salt it formed with muriate of soda.

To analyse the compound of steel with iridium and osmium, the alloy should be acted on by dilute sulphuric acid, and the residuum boiled in the acid ; the powder left is to be collected and heated with caustic soda in a silver crucible to dull redness for a quarter of an hour, the whole to be mixed with water, and having had excess of sulphuric acid added, it is to be distilled, and that which passes over condensed in a flask : it will be a solution of oxide of osmium, will have the peculiar smell belonging to that substance, and will give a blue precipitate with tincture of galls. The portion in the retort being then poured out, the insoluble part is to be washed in repeated portions of water, and then being first slightly acted on by muriatic acid to remove the iron, is to be treated with nitro-muriatic acid, which will give a muriate of iridium.

In these analyses, an experienced eye will frequently

perceive, on the first action of the acid, the presence of the alloying metal. When this is platina, gold, or silver, a film of the metal is quickly formed on the surface of the acid.

Of alloys of platina, palladium, rhodium, and iridium and osmium, a ready test is offered when the point is not to ascertain what the metal is, but merely whether it be present or not. For this purpose we have only to compare the action of the same acid on the alloy and on a piece of steel; the increased action on the alloy immediately indicates the presence of the metal; and by the difference of action, which on experience is found to be produced with the different metals, a judgment may be formed even of the particular one present.

The order in which the different alloys stand with regard to this action, is as follows: steel, cromium alloy, silver alloy, gold alloy, nickel alloy, rhodium alloy, iridium and osmium alloy, palladium alloy, platina alloy. With similar acid the action on the pure steel was scarcely perceptible; the silver alloy gave very little gas, nor was the gold much acted on. All the others gave gas copiously, but the platina alloy in most abundance.

In connection with the analysis of these alloys, there are some very interesting facts to be observed during the action of acids on them, and perhaps none of these are more striking than those last referred to. When the alloys are immersed in diluted acid, the peculiar properties which some of them exhibit, not only mark and distinguish them from common steel, and from each other, but also give rise to some considerations on the state of particles of matter of different

kinds when in intimate mixture or in combination, which may lead to clearer and more perfect ideas on this subject.

If two pieces, one of steel, and one steel alloyed with platina, be immersed in weak sulphuric acid, the alloy will be immediately acted on with great rapidity and the evolution of much gas, and will shortly be dissolved, whilst the steel will be scarcely at all affected. In this case, it is hardly possible to compare the strength of the two actions. If the gas be collected from the alloy and from the steel for equal intervals of time, the first portions will surpass the second some hundreds of times.

A very small quantity of platina alloyed with steel confers this property on it: $\frac{1}{400}$ increased the action considerably; with $\frac{1}{200}$ and $\frac{1}{100}$ it was powerful; with 10 per cent. of platina it acted, but not with much power; with 50 per cent. the action was not more than with steel alone; and an alloy of 90 platina with 20 steel was not affected by the acid.

The action of other acids on these alloys is similar to that of sulphuric acid, and is such as would be anticipated: dilute muriatic acid, phosphoric acid, and even oxalic acid, acted on the platina alloy with the liberation of more gas than from zinc; and tartaric acid and acetic acid rapidly dissolved it. In this way chalybeate solutions, containing small portions of protoxide of iron, may be readily obtained.

The cause of the increased action of acids on this and similar alloys, is, as the President of this Society suggested to us, probably electrical. It may be considered as occasioned by the alloying metal existing in such a state in the mass, that its particles form voltaic combinations with the particles of

steel, either directly, or by producing a definite alloy, which is diffused through the rest of the steel; in which case the whole mass would be a series of such voltaic combinations: or it may be occasioned by the liberation, on the first action of the acid, of particles which, if not pure platina, contain, as has been shown, a very large proportion of that metal, and which, being in close contact with the rest of the mass, form voltaic combinations with it in a very active state: or, in the third place, it may result from the iron being mechanically divided by the platina, so that its particles are more readily attacked by the acid, analogous to the case of proto-sulphuret of iron.

Although we have not been able to prove by such experiments, as may be considered strictly decisive, to which of these causes the action is owing, or how much is due to any of them, yet we do not hesitate to consider the second as almost entirely, if not quite, the one that is active. The reasons which induce us to suppose this to be the true cause of the action, rather than any peculiar and previous arrangement of the particles of steel and platina, or than the state of division of the steel, are, that the two metals combine in every proportion we have tried, and do not, in any case, exhibit evidences of a separation between them, like those, for instance, which steel and silver exhibit; that when, instead of an acid, weaker agents are used, the alloy does not seem to act with them as if it was a series of infinitely minute voltaic combinations of steel and platina, but exactly as steel alone would do; that the mass does not render platina wire more negative than steel, as it probably in the third case would do; that it does not rust more rapidly in a damp atmosphere; and that when

placed in saline solutions, as muriate of soda, &c., there is no action takes place between them. In such cases it acts just like steel; and no agent that we have as yet tried, has produced voltaic action that was not first able to set a portion of the platina free by dissolving out the iron.

Other interesting phenomena exhibited by the action of acid on these steels, are the differences produced when they are hard and when soft. Mr. DANIEL, in his interesting paper on the mechanical structure of iron, published in the Journal of Science, has remarked, that pieces of hard and soft steel being placed in muriatic acid, the first required five fold the time of the latter to saturate the acid; and that when its surface was examined, it was covered with small cavities like worm-eaten wood, and was compact and not at all striated, and that the latter presented a fibrous and wavy texture.

The properties of the platina alloy have enabled us to observe other differences between hard and soft steel equally striking. When two portions of the platina alloy, one hard and one soft, are put into the same diluted sulphuric acid and suffered to remain for a few hours, then taken out and examined, the hard piece presents a covering of a metallic black carbonaceous powder, and the surface is generally slightly fibrous, but the soft piece, on examination, is found to be covered with a thick coat of grey metallic plumbaginous matter, soft to the touch, and which may be cut with a knife, and its quantity seven or eight times that of the powder on the hard piece: it does not appear as if it contained any free charcoal, but considerably resembles the plumbaginous powder Mr. DANIEL describes as obtained by the action of acid on cast iron.

The same difference is observed if pure steel be used, but it is not so striking; because, being much less rapidly attacked by the acid, it has to remain longer in it, and the powder produced is still farther acted on.

The powder procured from the soft steel or alloy in these experiments, when it has not remained long in the acid, exactly resembles finely divided plumbago, and appears to be a carburet of iron, and probably of the alloying metal also. It is not acted on by water, but in the air the iron oxidates and discolours the substance. When it remains long in the acid, or is boiled in it, it is reduced to the same state as the powder from the hard steel or alloy.

When any of these residua are boiled in diluted sulphuric or muriatic acid, protoxide of iron is dissolved, and a black powder remains unalterable by the farther action of the acid; it is apparently in greater quantity from the alloys than from pure steel, and when washed, dried, and heated to 300° or 400° in the air, burns like pyrophorus, with much fume; or if lighted, burns like bitumen, and with a bright flame; the residuum is protoxide of iron, and the alloying metal. Hence, during the action of the acid on the steel, a portion of hydrogen enters into combination with part of the metal and the charcoal, and forms an inflammable compound not acted upon by the acid.

Some striking effects are produced by the action of nitric acid on these powders. If that from pure steel be taken, it is entirely dissolved; and such is also the case if the powder be taken from an alloy, the metal of which is soluble in nitric acid; but if the powder is from an alloy, the metal of which is not soluble in nitric acid, then a black residuum is left not

touched by the acid; and which, when washed and carefully dried, is found, when heated, to be deflagrating; and with some of the metals, when carefully prepared, strongly explosive.

The fulminating preparation obtained from the platina alloy, when dissolved in nitro-muriatic acid, gave a solution containing much platina, and very little iron. When a little of it was wrapped in foil and heated, it exploded with much force, tearing open the foil, and evolving a faint light. When dropped on the surface of heated mercury, it exploded readily at 400° of Fahrenheit, but with difficulty at 370°. When its temperature was raised slowly, it did not explode, but was decomposed quietly. When detonated in the bottom of a hot glass tube, much water and fume were given off, and the residuum collected was metallic platina with a very little iron and charcoal. We are uncertain how far this preparation resembles the fulminating platina of Mr. Edmund Davy.

In these alloys of steel the differences of specific gravity are not great, and may probably be in part referred to the denser state of the metals from more or less hammering; at the same time it may be observed, that they are nearly in the order of the specific gravities of the respective alloying metals.

The alloys of steel with gold, tin, copper, and chromium, we have not attempted in the large way. In the laboratory, steel and gold were combined in various proportions; none of the results were so promising as the alloys already named, nor did either tin or copper, as far as we could judge, at all improve steel. With titanium we failed, owing to the imperfection of crucibles. In one instance, in which the fused

button gave a fine damask surface, we were disposed to attribute the appearance to the presence of titanium; but in this we were mistaken; the fact was, we had unintentionally made wootz. The button, by analysis, gave a little silex and alumine, but not an atom of titanium; menachanite, in a particular state of preparation, was used: this might possibly contain the earths or their basis, or they may have formed a part of the crucible.

M. BERTHIER, who first made the alloy of steel and chromium,* speaks very favourably of it. We have made only two experiments. 1600 grains of steel, with 16 of pure chrome, were packed into one of the best crucibles, and placed in an excellent blast furnace: the metals were fused, and kept in that state for some time. The fused button proved good and forged well: although hard, it showed no disposition to crack. The surface being brightened, and slightly acted on by dilute sulphuric acid, exhibited a crystalline appearance; the crystals, being elongated by forging, and the surface again polished gave, by dilute acid, a very beautiful damask. Again, 1600 grains of steel with 48 of pure chrome were fused: this gave a button considerably harder than the former. This too was as malleable as pure iron, and also gave a very fine damask. Here a phenomenon rather curious was observed: the damask was removed by polishing, and restored by heat without the use of any acid. The damasked surface, now coloured by oxidation, had a very novel appearance: the beauty was heightened by heating the metal in a way to exhibit all the colours caused by oxidation, from pale straw to blue, or from about 430 to 600° of FAHRENHEIT. The blade

* Annales de Chimie, XVII. 55.

of a sabre, or some such instrument, made from this alloy, and treated in this way, would assuredly be beautiful, whatever its other properties might be; for of the value of the chrome alloy for edge tools we are not prepared to speak, not having made trial of its cutting powers. The sabre blade, thus coloured, would amount to a proof of its being well tempered; the blue back would indicate the temper of a watch spring, while the straw colour towards the edge would announce the requisite degree of hardness. It is confessed, that the operation of tempering any blade of considerable length in this way, would be attended with some difficulty.

In the account now given of the different alloys, only one triple compound is noticed, namely, steel, iridium and osmium; but this part of the subject certainly merits farther investigation, offering a wide and interesting field of research. Some attempts to form other combinations of this description proved encouraging, but we were prevented, at the time, by various other avocations, from bestowing on them that attention and labour they seemed so well to deserve.*

It is a curious fact, that when pure iron is substituted for steel, the alloys so formed are much less subject to oxidation. 3 per cent. of iridium and osmium fused with some pure iron, gave a button, which when forged and polished was exposed, with many other pieces of iron, steel, and alloys, to a moist atmosphere: it was the last of all showing any rust. The colour of this compound was distinctly blue; it had the property of becoming harder when heated to redness and quenched in a cold fluid. On observing this steel-like character, we sus-

* It is our intention to continue these experiments at every opportunity, but they are laborious, and require much time and patience.

pected the presence of carbon; none however was found, although carefully looked for. It is not improbable that there may be other bodies, besides charcoal, capable of giving to iron the properties of steel; and though we cannot agree with M. Boussingault,* when he would replace carbon in steel by silica or its base, we think his experiments very interesting on this point, which is worthy farther examination.

We are not informed as to what extent these alloys, or any of them, have been made at home, or to what uses they have been applied; their more general introduction in the manufacture of cutlery would assuredly add to the value, and consequently to the extension of that branch of trade. There are various other important uses to which the alloys of steel may advantageously be applied. If our information be correct, the alloy of silver, as well as that of platina, has been to some considerable extent in use at His Majesty's Mint. We do know, that several of the alloys have been diligently and successfully made on the Continent; very good specimens of some of them having been handed to us; and we are proud of these testimonies of the utility of our endeavours.

To succeed in making and extending the application of these new compounds, a considerable degree of faithful and diligent attention will be required on the part of the operators. The purity of the metals intended to form the compound is essential; the perfect and complete fusion of both must, in every case, be ascertained: it is farther requisite, that the metals be kept for some considerable time in the state of thin fusion; after casting, the forging is with equal care to be attended to; the metal must on no account be overheated;

* Annales de Chimie, XVI. 1.

and this is more particularly to be attended to when the alloying metal is fusible at a low temperature, as silver. The same care is to be observed in hardening: the article is to be brought to a cherry-red colour, and then instantly quenched in the cold fluid.

In tempering, which is best performed in a metallic bath properly constructed, the bath will require to be heated for the respective alloys, from about 70° to 100° of Fahrenheit above the point of temperature required for the best cast steel. We would farther recommend, that this act of tempering be performed twice; that is, at the usual time before grinding, and again just before the last polish is given to the blade. This second tempering may perhaps appear superfluous, but upon trial its utility will be readily admitted. We were led to adopt the practice by analogy, when considering the process of making and tempering watch springs.

XXI. *Some observations on the buffy coat of the Blood, &c. By* John Davy, *M. D. F. R. S.*

Read April 18, 1822.

1. The buffy coat, as it is technically called, which appears on blood drawn from persons labouring under inflammatory disease, has been referred, by Mr. Hewson, to two circumstances; to increased tenuity of the blood, and to its slow coagulation.* But, in the explanation which is now usually given of the phenomenon, it is attributed to the latter circumstance, to the entire neglect of the former. The reverse of this, I believe, would be more correct; for in cases in which the inflammatory diathesis is best marked, the separation of the red particles from the blood drawn is most rapid, often occurring in one or two minutes; and, in some diseases, particularly in erysipelas, the blood taken from a vein coagulates as rapidly as healthy blood, and yet exhibits the buffy coat. In instances of this kind, when I have watched the coagulation of the blood, the red particles have subsided in the short space of two minutes, leaving a supernatant stratum of coagulable lymph, transparent and liquid. The buffy coat, in these instances, did not appear on the blood collected in the common bleeding cups, only when small vessels, as wine-glasses, or small gallipots were used, and quickly filled, and instantly set aside to rest. May it not, therefore, be inferred generally,

* "An Experimental Inquiry into the Properties of the Blood, with Remarks on some of its Morbid Appearances." By William Hewson, F. R. S. London, 1771, p. 56 and 59.

that the buffy coat is principally owing, not to the slow coagulation of the blood on which it appears, but to its increased tenuity; or, in other words, to the diminished viscidity of coagulable lymph, the effect of morbid vascular action connected with the inflammatory diathesis?

Mr. HEY has asserted, in opposition to Mr. HEWSON, that the coagulable lymph is not itself attenuated in inflammatory diseases, and that when it appears to be so, it is from dilution with serum.* Were this opinion correct, such blood should be of low specific gravity, which it is not, as I have satisfied myself by numerous experiments, made both in this country and in Ceylon. In general, I have found the blood, on which a buffy coat has appeared, of higher specific gravity than healthy blood.

2. It is an opinion pretty generally prevalent, that the age of these morbid adhesions, which are so frequently met with in the dissection of bodies, connecting together serous membranes, may be guessed at by their strength: thus, weak adhesions are usually considered of recent origin, and firm adhesions, of long standing. Is this opinion correct? And, does it agree with the properties of coagulable lymph, of which these adhesions are principally formed? Many circumstances, of which it will be sufficient to mention a few, lead to a reply in the negative.

Wounds, it is well known, that heal by the *first intention*, are often firmly united in twenty-four hours.

I have observed strong adhesions formed in the same space of time between the surfaces of the pleura, in consequence of inflammation artificially excited. An instance may be given.

* Observations on the Blood, by WILLIAM HEY, F. R. S. London, 1779, p. 47-49.

At Colombo, in the island of Ceylon, in January, 1819, I made the following experiment on a young dog nearly full grown. An opening was made with a scalpel between the ribs of the right side of the chest, through which about a scruple of arrack was injected into the cavity of the pleura. The lung was slightly wounded; air passed freely through the opening, and a little frothy blood was discharged. The animal, at first, seemed to suffer much pain, and to be very languid; but, left to itself, it gradually recovered, and in the course of the day took some food. At the expiration of twenty-four hours it was hot, but apparently not suffering pain; it was running about, and the wound was closed. It was now killed, and almost immediately examined. A good deal of vascular coagulated lymph was found under the skin round the wound, connecting the cutis and the intercostal muscles; the adjoining cellular membrane was inflamed; some bloody serum was effused into the right cavity of the chest; many pretty firm and long adhesions had formed between the pleura pulmonalis and costalis, both which were of a reddish hue; there were many adhesions too between the pleura and pericardium; and the pericardium was inflamed, and generally adherent to the surface of the heart.

The coagulated lymph of the buffy coat of the blood may be used as an illustration and confirmation of the short time in which strong adhesions may form. Liquid, when the blood is drawn, coagulable lymph gradually becomes, first viscid, and afterwards solid. In the viscid state, as I have frequently observed, when it is still transparent, it has the tenacity of mucus, and admits of being drawn out into fibres and bands, which, soon becoming solid and opaque, very well represent

the ordinary adhesions of the lungs; and in a very few hours attain their maximum of strength.

This viscidity, which coagulable lymph acquires in passing from a liquid to a solid form, has not, that I am aware, been noticed by authors; and the formation of adhesions is usually explained without reference to this quality.*

Though I believe the common opinion to be untenable, that the age of adhesions can be decided by their strength, it is far from my intention to maintain that they do not become firmer in progress of time, or that their duration may not occasionally be conjectured from their appearance and resistance.

3. It is believed by many, that the small portions of serous fluid which are found after death in the cavities of serous membranes, especially in the pericardium and the ventricles of the brain, may have been poured out after the cessation of life.† I am not aware that this opinion is other than hypothetical, or that it is supported by any precise facts. As a theoretical conclusion its correctness seems doubtful. I have endeavoured to put it to the test of experiment, and the result has not been favourable to it. I have notes of three different experiments on dogs, which were made in Ceylon in 1818, all which seem to show that, under ordinary circumstances, no effusion of serum, or exudation so as to occasion accumulation, takes place after death. The experiments were briefly the following. In each instance, a healthy dog was

* Vide "The Morbid Anatomy of some of the most important Parts of the Human Body, by MATTHEW BAILLIE, M. D. F. R. S. &c." 5th edit. p. 6.

† SAUVAGES' Nosologia Method. *Ephialtes ex Hydrocephalo.*

Cours d'Anatomie Medicale, par ANTOINE PORTAL, tom. IV. p. 54. 8vo. Paris, 1803.

suddenly killed by a blow on the occiput; the cavity of the chest was instantly laid open, and the pericardium inspected. A small quantity of serum was found in it, which was removed with a sponge, and the incisions made were closed by sutures. At the end of twenty-four hours the sutures were divided, and the pericardium was again examined. Not a single drop of fluid had collected in it, in any instance, though in two of the trials the right auricle and ventricle were considerably distended with blood.

If these results be conclusive against fluid being effused into the pericardium after death in dogs, the conclusion from them, admits of being extended by analogy to other cavities of the same texture, and to man; and I am not acquainted with any pathological observations in opposition to it. The discovery of serous effusions in examinations *post mortem*, no symptoms of their occurrence or existence having been noticed during life, is surely no evidence of their having taken place after the cessation of vital action. It is too well known to be insisted on, that large portions of fluid may accumulate in the pericardium, and even in the ventricles of the brain, without a single symptom to indicate the fact.

Fort Pitt, Chatham,
March 5, 1822.

XXII. *On the mechanism of the Spine.* *By* Henry Earle, *Esq. F. R. S. Surgeon to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital.*

Read April 25, 1822.

Having been lately engaged in examining the structure of the vertebræ in different animals, I have been particularly struck with the mechanism of the spine and spinal canal in birds, by which a most remarkable degree of motion is gained in the neck, without any injury or pressure on a part of such vital importance to the existence of animal life, as the spinal marrow; an extent of motion, so great indeed as completely to compensate for the deficiency of it in the dorsal and lumbar regions, as well as for the want of any prehensile power in the anterior extremities. In attempting to explain the nature of this peculiar mechanism, which tends to throw considerable light on the physiology and pathology of the spine, I believe that I have not been preceded by any author. The cervical vertebræ in birds are very numerous, varying from nine to twenty-four.* They differ considerably from one another, according to their situation, in the form and direction of their articulating surfaces, and in the number and shape of

* This great diversity in the number of the cervical vertebræ in birds, is the more remarkable, when contrasted with the uniformity which pervades the class mammalia, where the number (with one single exception, the three-toed sloth) is constantly seven. The mole, whose head appears lost between the scapulæ, has precisely the same number as the giraffe and the horse.

the different processes, which afford extensive means of attachment to the muscles concerned in the different motions of the neck. Unlike the vertebræ in man and most of the mammalia, they are articulated together by complicated joints, which bear a close resemblance to the articulation of the olecranon with the humerus in the human subject, but differing in some respects; the vertebræ in birds admitting of lateral motion as well as flexion and extension, whilst the elbow is strictly a hinge-like joint.

The varying position of these articular surfaces is greatly favoured by the interposition of a cartilage, which is curiously adapted to the surface of each bone, and is enclosed between reduplications of synovial membrane; and thus each joint is double, consisting of two synovial cavities, and is analogous to the articulation of the lower jaw in man; a circumstance, I believe, not mentioned either by CUVIER, BLUMENBACH, or MACARTNEY.

The canal of each vertebra is of very unequal calibre, the centre being narrowest. It enlarges above and below, and at each joint is nearly three times the capacity that it is in the centre; and thus the canal of each individual vertebra may not unaptly be compared to an hour-glass. The canal is closed in front by the posterior surfaces of the bodies of the vertebræ, but behind it is very imperfect; and in the skeleton there is a large lozenge-shaped opening, formed by the diverging inferior articular processes and the converging plates which unite to form the back of the canal. This, in a recent state, is filled up by a membrane, and is protected by the highly elastic and powerful ligamentum nuchæ.

This mechanism, besides allowing of the greatest possible

freedom of motion, appears to be intended, at the same time, to guard against the possibility of any undue pressure on the spinal marrow. This is very readily demonstrated by removing the ligamentum nuchæ and membrane which closes the above mentioned opening. The spinal marrow, enveloped in its membranes, will immediately come in view. Its outer membrane is very vascular, and of a delicate structure, and is connected with the canal by a fine filamentous cellular substance; it is larger than the inner membrane, with which it is but loosely connected.

When the spinal canal has been thus exposed, the individual vertebræ may be bent backward to a right angle, and laterally to an angle of 45°, without in the least compressing the marrow which occupies so small a space of the whole calibre of the canal at each articulation, as to be quite secured from any injury from this motion. The design, in this structure, becomes even yet more obvious, on viewing the whole extent of the spinal cord. It is nearly of the same size throughout, diminishing very gradually from above downwards, and completely occupies the narrow central part of the canal of each cervical vertebra, where no motion can affect it. The same may be observed in that part of the spine which corresponds to the dorsal and lumbar divisions, which in birds do not admit of motion; for here we find no variation, either in the spinal canal or the marrow, except where the numerous branches are given off to form the great sciatic plexus, to supply the lower extremities, where it swells out into a bulbous shape, corresponding to the cavity in the bone.

Before quitting the subject of the spine in birds, it will be

right to mention one more peculiarity, apparently connected with the same mechanism. Contrary to the usual course in other animals, the nerves that are given off from the cervical portion of the spinal marrow, pass obliquely upwards at a considerable angle, through an opening between the root of the inferior articulating process, and the body of the bone; they then divide, and one branch descends through the opening in the lateral process, and the other branch is distributed to the surrounding muscles and integuments.

One principal object in comparative anatomy, or rather in comparative physiology, is to enable us, by examining particular structures, which are more developed in some animals, and in whom consequently the functions of such structures are more apparent, to judge of the probable uses of similar structures existing in a diminished proportion in other animals. On investigating this subject, and examining the spines of several other animals, I have found a similar arrangement, varying only in degree, and that exactly in proportion to the extent of motion permitted between the vertebræ.*

In the formation of the spine in man, it was requisite to combine two very opposite qualities. The solidity and strength of a column were required to be united with the flexibility necessary to the performance of our diversified actions. To attain

* This rule will be found to hold good, even in those animals which form exceptions, with respect to the general form and arrangement of the different processes. Thus, in the mole, whose cervical vertebræ are mere bony rings without any spinous processes, and which, consequently, admit of extensive motion, the canal is remarkably capacious. In the bat, whose dorsal vertebræ are either wholly without spinous processes, or have only short tubercles, the canal, at this part, is of greater volume than either in the cervical or lumbar vertebræ; and, contrary to the general rule, this division of the spine admits of considerable motion.

these various ends, this beautiful structure is admirably adapted. The broad horizontal planes afforded by the bodies of the vertebræ, the mechanical locking of the articular processes, and the powerful ligamentous bands which unite them, so connect the whole as to form one column, whilst the numerous articulations into which it is subdivided, which are separated by masses of highly elastic matter, at once interrupt the effect of concussion, and allow a slight extent of yielding of one vertebra upon the other. The motions of the individual vertebra are obscure and limited, but the aggregate of the whole is considerable. The extent of motion varies in each region; in the back, every thing conspires to limit it; but in the neck and loins it is much greater; and in sawing open the spinal canal, we find a very similar provision to that which I have before described in birds, namely, an exact correspondence between the extent of motion permitted, and the size and form of the canal. Thus, in the dorsal division, where motion hardly exists, its calibre is less; it is of a rounded form, and it is more closely adapted to the size of its contents. In the superior cervical vertebræ, where the extent of motion is greater, the canal is of a triangular form, and is considerably larger in proportion to the spinal cord. In the lumbar vertebræ it is also triangular, and much more capacious than in the dorsal. Obviously, with the same intention, the theca is very loosely connected with the bony canal, and a considerable space is left between it and the other membranes, to allow of a sufficient play of one surface on the other, so that at the greatest extent of natural curve, no perceptible stretching of the marrow can take place, which would be liable to continual pressure, if, closely enveloped in its

membranes, it completely filled the canal, in every motion of which it must, in that case, participate.

To afford additional support to the marrow which this loose state of membranes would leave very insecure, if enclosed in so delicate a tissue as the pia mater of the brain, this membrane, which may be considered as the proper tunic of the marrow, is greatly thickened, and partakes more of the characters of a fibrous membrane.

The membranous band, which has been termed the ligamentum denticulatum, appears to be superadded to restrain the lateral motions of the marrow, and to steady it in the canal. By these membranous processes, the marrow may not, improperly, be said to be lashed to the sides of the spinal sheath, in which, from the disproportion between them, it would otherwise be liable to perpetual variation of position, and pressure from the bony parietes. That a certain degree of freedom of motion between the membranes is essential to the due performance of the functions of the spinal marrow, is proved by the effect of accidents and disease. It would be out of place here, to bring forward a detail of particular cases, but I may mention briefly, that I have ascertained, by dissection, that the most distressing train of nervous symptoms, and even complete paraplegia, may be produced by adhesions taking place between the membranes, and by effusion into the canal or theca.

In conclusion I may observe, that this view of the subject tends to throw considerable light on the pathology of the spine, and assists in explaining a circumstance which I have repeatedly noticed in diseases affecting the vertebræ, namely, that the symptoms of irritation and inflammation of the spinal

marrow, are much more early manifested, and are generally far more serious in their consequences when the dorsal vertebræ are affected, than when either the cervical or lumbar are the seat of disease. In the former case, the slightest congestion or effusion is often productive of serious symptoms, from the canal being smaller and more completely filled with the marrow and its membranes; whilst, in the latter description of cases, from the greater capacity of the canal and looseness of the membranes, considerable effusion may exist, without, at first, producing any marked symptoms, more particularly in the lumbar region, where other circumstances concur in rendering the effect of pressure less sensibly felt; to enter into a description of which, would be foreign to the object of this paper.

As it is difficult to convey any clear idea of complicated forms by words, I have subjoined a sketch of some cervical vertebræ in birds, with a description of the different parts.

EXPLANATION OF PLATE XXX.

Figure 1. Represents an anterior view of a single cervical vertebra.

Fig. 2. A posterior view of the same.

Fig. 3. A lateral view of the same.

Fig. 4. A front view of two vertebræ articulated together.

Fig. 5. A back view of the same.

The letters of reference are the same in all the figures.

A. The body of the vertebra.

BB. lateral processes.

CC. Styloid processes, with tubercles near their base, which restrain the motion of the vertebræ in the anterior direction.

In some birds, at the upper part of the neck, a bony arch extends from one to the other, to preserve the blood vessels which pass under it from pressure. This is the case in the heron, and other very voracious birds.

D. Lunated excavation for articulation with the superior vertebra.

E. Semi-lunar convexity, corresponding to the excavation above mentioned.

F F. Diverging inferior articular processes, with surfaces facing obliquely outward.

G G. Superior articular surfaces, facing obliquely inwards.

H. I. Spinal canal, imperfect in the skeleton, in consequence of the vacancy left between the diverging inferior articular processes and the superior converging plates, which unite to form the back of the canal K.

L. Opening for the passage of the spinal nerves.

M. Foramen for the passage of the carotid artery and branches of the spinal nerves.

Fig. 6. A perpendicular section of two vertebræ, showing the interarticular cartilages.

28, *George Street, Hanover Square,*
April 2, 1822.

XXIII. *Of the Nerves which associate the muscles of the Chest, in the actions of breathing, speaking, and expression. Being a continuation of the paper on the Structure and Functions of the Nerves. By* Charles Bell, *Esq. Communicated by Sir* Humphry Davy, *Bart. LL. D. P. R. S.*

Read May 2, 1822.

In a former paper an examination was made of the nerves of the face; that part of the system was taken, as proving in a manner the least liable to exception, that two sets of nerves, hitherto undistinguished, possessed distinct powers; and that very different effects were produced when the muscles and integuments were deprived of the controuling influence of the one or of the other of these nerves. In that paper it was shown, that parts remote in situation, were yet united by the closest sympathy with the lungs. That by a division of one nerve, these organs could be severed from the other parts of the apparatus of respiration; and though rendered dead to the influence of the heart and lungs, were yet possessed of their other properties, such as sensibility and voluntary motion.

In the present paper it is proposed to prosecute this subject, by tracing the nerves which influence the motions of the trunk of the body in respiration, and to subject them to a similar enquiry.

It is an encouraging circumstance to the Author of this Paper, and may incline the Society to bear with the detail

into which it will be necessary to enter, that already practical benefits have arisen from the former paper; that the views presented there, as connected with general science, being carried into practice, have enabled the physician to make more accurate distinctions of disease, and the surgeon, in removing deformity, to avoid producing distortion.

Of the motions of the thorax, as affording a key to the intricacy of its nerves.

We have seen the necessity of considering all the functions and relations of a part of the animal machine, the nerves of which we propose to distinguish according to their uses; and this is even more necessary with respect to the thorax than the face. This will be evident, if we make a mere catalogue of the uses of this compages of bones and muscles. Besides affording support and protection to the heart and lungs, and the viscera of the higher region of the abdomen, the thorax performs these offices:

1. It alternately opposes and yields to the weight of the atmosphere, thus producing respiration.

2. In addition to the uniform motion of the chest in breathing, there is the occasional increase and agitation commensurate to the excited state of the animal frame, when additional muscles are brought into action.

3. There is the exertion of the respiratory apparatus in natural voice, and in articulate language.

4. Through the nerves and muscles employed in respiration, are also exhibited the emotions and passions of the mind.

5. The organs of the sense of smelling, and particularly the muscles which move the cartilages of the nose, are, in their exercise, as necessarily joined to the act of inspiration, as those of speech are to the act of expiration.

6. The powers of the arms in voluntary exertion, are in a great measure dependent upon the expansion of the thorax; so that the act of inspiration is always combined with sudden and powerful exertion. The more indeed we attend to the motions of the frame, whether in efforts of strength, or in the act of respiration, the more remarkable will the unexpected combinations of the muscles appear.

It is only when we are made sensible of the extent of the respiratory actions, and that they in effect extend over the whole face and neck and trunk, that we can comprehend how the mechanism of the thorax, or rather of the respiratory apparatus generally, affects the arrangement of the whole nervous system. Wherever, in examining the comparative anatomy of animals, we find ribs rising and falling by respiratory muscles, we have a *medulla spinalis*, and the distinction of *cerebrum* and *cerebellum*. And experiment and observation prove, that the seat of that power which controuls the extended act of respiration, is in the lateral portions of the *medulla oblongata*, from which it is continued through certain respiratory nerves which pass out from the neck, and also downwards, by corresponding columns of the spinal marrow, to the intercostal nerves.

Origins of the respiratory nerves.

The nerves on which the associated actions of respiration depend, and which have been proved to belong to this system, by

direct experiment, and the induction from anatomy, arise very nearly together. Their origins are not in a bundle, or fasciculus, but in a line or series, and from a distinct column of the spinal marrow. Behind the *corpus olivare*, and anterior to that process which descends from the cerebellum, the *corpus restiforme*, a convex strip of medullary matter, may be observed, and this convexity, or fasciculus, or *virga*, may be traced down the spinal marrow, betwixt the sulci, which give rise to the anterior and posterior roots of the spinal nerves.

This portion of medullary matter is narrow above where the *pons Varollii* overhangs it. It expands as it descends; opposite to the lower part of the *corpus olivare* it has reached its utmost convexity, after which it contracts a little, and is continued down the lateral part of the spinal marrow.

From this track of medullary matter on the side of the *medulla oblongata*, arise in succession from above downwards, the *portio dura* of the seventh nerve: the *glosso-pharyngeus* nerve: the nerve of the *par vagum*: the *nervus ad par vagum accessorius*: the *phrenic*, and the *external* respiratory nerves.

It is probable that the branches of the intercostal and lumbar nerves, which influence the intercostal muscles and the muscles of the abdomen in the act of respiration, are derived from the continuation of the same cord or slip of medullary matter. Nor will it escape observation, that the nerves called phrenic and external respiratory, though coming out with the cervical nerves, do, in all probability, take their origin from the same portion of the medulla spinalis with the accessory nerve.

The intercostal nerves, by their relations with the medulla oblongata, are equal to the performance of respiration, as it

regards the office of the lungs; but they are not adequate to those additional functions which are in a manner imposed upon the respiratory apparatus, when they are brought to combine in other offices.

Of the muscles of the trunk, which are brought in aid of the common respiratory muscles.

If we look upon the frame of the body for the purpose of determining which are the muscles best calculated to assist in the motions of the chest, when there is an increased or excited action, we shall have little difficulty in distinguishing them, and we shall have as little hesitation in assigning a use to the nerves which supply these muscles exclusively. For these nerves have the same origin: they take an intricate course, threading and passing betwixt other nerves and other muscles, to be entirely given to the muscles which heave the chest.

In this enquiry it is necessary to observe, that the life of animals is protected by a particular sense which gives rise to an instinctive motion of drawing the breath, and by which the chest is suddenly and powerfully expanded on exertion or alarm. The start on sudden alarm, is accompanied with a rapid expansion and rising of the chest, and the voice, at such a moment, is produced by suddenly inhaling, and not by expiration; and this expansion of the chest combines with the preparation for flight or defence, since the extension of the muscles lying on the breast and back is produced by this motion, and since they are thereby rendered more powerful in their influence upon the arms or anterior extremities. It cannot escape observation, that oppression and difficulty of

breathing is exhibited in gasping and forcible inspiration, in drawing the breath, not in throwing it out.

Accordingly, when we examine the trunk of the human body, we have no difficulty in distinguishing the muscles most capable of raising the chest; and these in effect, we see powerfully influenced in deep inspiration, whether the action be voluntary, as in speech, or involuntary, as in the last efforts of life, when sense is lost. They are the mastoid muscle, the trapezius, the serratus magnus, and the diaphragm.

1. *Sterno-cleido-mastoideus.** This muscle, by its attachment to the sternum or breast bone, raises or heaves the chest; and the operation of this muscle is very evident in all excited states of respiration, in speaking, and still more in singing, coughing, and sneezing. But there is something necessary to the full effect of this muscle on the chest, for otherwise it will be a muscle of the head, and not of the chest.

2. *The trapezius* † must fix the head or pull it backwards before the *mastoideus* can act as a respiratory muscle, and how they are combined we shall presently see. The position of the head of the asthmatic, during the fit, as well as the posture of the wounded or the dying, prove the influence of the upper part of the trapezius in excited respiration.

The trapezius has a still more powerful and important influence in respiration when the action rises above the ordinary condition, and that is by drawing back the scapula, to give the necessary effect to the action of the serratus magnus.

3. The *serratus magnus anticus* ‡ being extended over the whole side of the chest, and attached in all the extent from the second to the eighth rib, is very powerful in raising the

* See Plate XXXI. fig. 2, A. † Fig. 2, B. B. ‡ Fig. 2, E. E.

ribs; but it cannot exert this power, independently of the trapezius, since, without this combination, its force would be exerted in moving the scapula, and not the ribs; unless the scapula be fixed, or pulled back by the *trapezius*, the *serratus* is not a muscle of respiration.

In this manner do these three powerful muscles hang together in their action, combining with the diaphragm to enlarge the cavity of the chest in all its diameters.

The course of our enquiry leads us to ask, are these muscles privileged above others by any peculiarity of nerves? And the answer is plain: to these muscles alone, are the nerves, which I am about to call respiratory nerves of the chest, distributed.

*Anatomy of the respiratory nerves of the trunk.**

The nerves which give rise to the extraordinary intricacy of this system on the side of the neck, are the spinal accessory nerve, the phrenic nerve, and the external thoracic nerve. By reference to any common book of anatomy, the phrenic nerve (4 Fig. 2.) will be found to have its great root or origin from the fourth cervical nerve; and there joins this, a more slender branch from the third cervical nerve. But, besides these roots, it has connections, which of themselves would mark the relations of the nerve; high in the neck, it is connected with the *nervus vagus*, and with the *lingualis medius*, while, at the same time, a branch is given off to the muscles of the larynx. The trunk of the nerve descends into the cavity of the thorax, and gives no branches until arriving at the diaphragm (Fig. 2.G), it sends out numerous diverging branches, which are lost in the substance of that muscle.

* See Plate XXXI. Fig. 1 and 2.

It has been long known that irritation of this nerve convulses the diaphragm, and that cutting it across paralyses that muscle. These facts, with the consideration of its course, prove it to be a respiratory nerve, and such has been the universal opinion.

But to what purpose should a distinct nerve be sent to the diaphragm, if the other muscles, seated externally, and which are associated in action with the diaphragm, and as important to respiration, were left without a similar tie to unite them with each other, and with the organs of the voice?

The external respiratory nerve of the thorax (5 Fig. II.) is a counterpart of the internal or phrenic nerve. It comes out from the 4th and 5th cervical nerves, and often it is connected with the phrenic. It diverges somewhat from that nerve, because, instead of descending within the chest, it falls over the ribs, and descends in a distinct flat trunk upon the outside of the chest, to be distributed intirely to the *serratus magnus anticus*. This muscle has nerves from the spinal marrow, because it has to combine in the motions of the frame in loco-motion. But the long descending nerve is a respiratory nerve; which we may know from its origin, course, and destination; in its origin and course it is like the diaphragmatic nerve, and in its destination also, since it is given to a muscle necessary to full inspiration.

I come now to the *spinal accessory nerve* (Plate XXXI. fig. II. 3)* which is more particularly an object in this paper. It is called here the superior respiratory nerve of the trunk. Experiments may take a colour from the preconceived idea, but the accurate investigation of the structure will not deceive

* *Nervus ad par vagum accessorius.*

us. The author therefore entreats attention to the anatomy of this nerve, as leading in the most conclusive manner to a knowledge of its functions.

It arises from the cervical portion of the spinal marrow (Plate XXXI. fig. I. 4); but instead of collecting its branches to go out by the side of the vertebræ, like the internal and external respiratory nerves, it shoots upwards through the theca of the spinal marrow, enters the skull, and joins the 8th pair of nerves; from which it has its term of accessory. We see the roots of this nerve as far down as the 4th cervical nerve.* These roots arise neither from the posterior nor the anterior column of the spinal marrow, but betwixt the posterior roots of the cervical nerves and the *ligamentum denticulatum*, and from the *column of medullary matter* above described. The origins of this nerve come off in one line, and that line is in the direction of the roots of the eighth pair, and of that nerve which has been proved to be the respiratory nerve of the face. In its ascent the accessory nerve is attached to the posterior root of the first cervical nerve.

The nerve having ascended through the *foramen magnum*, passes out from the skull associated with the nerves constituting the *eighth pair*, and in the same sheath with them; they all go out through the *foramen lacerum*, and by the side of the jugular vein. In this course the accessory nerve divides into two. One of these divisions joins filaments of the *par vagum* (Fig. II. 6); and these again send nerves to the *glosso-pharyngeal* nerve (Fig. II. 2); and sometimes a branch may be seen going to the *lingualis medius*. The

* In the ass, its roots are seen to extend much lower down.

more exterior division of the accessory nerve descends behind the jugular vein, and comes forward and perforates the mastoid muscle, (Plate XXXI. fig. II. A). In its passage through the muscle it sends off branches which course through its substance; and if, as sometimes happens, though rarely, the nerve does not pass through the muscle, these branches are, notwithstanding, invariably given to it.

When the nerve has escaped from the back part of the mastoid muscle, it forms a communication with that branch of the 3d cervical nerve that ascends behind the muscle; and nearly at the same time it is joined by a branch from the 2d cervical nerve. The superior respiratory nerve now descends upon the neck, and begins to disperse its branches in regular order to the edge of the trapezius muscle; (Fig. II. B.) four or five branches take their course to that muscle, separate into minute subdivisions, and are lost in its substance. One more considerable division, being the lowest of these, is joined by a long descending branch of the 2d cervical nerve. Encreased by this addition, it descends under the trapezius and behind the clavicle. Following this descending branch, it will be found exclusively attached to the trapezius. Behind the scapula it is again joined by branches from the spinal nerves; and here a sort of imperfect plexus is formed, from which divisions of the nerve, still descending, follow the lower edge of the muscle, and are finally dispersed among its fibres.

This nerve arises from the same column with the respiratory nerves; it takes a most intricate and circuitous passage to form a junction with nerves which we know to belong to that class; it sends branches to join the nerves of the tongue

and pharynx; it sends branches to the larynx in company with the branches of the *par vagum;* it then crosses the great nerves of the neck, passes under the spinal nerves, goes to no other muscles in its course, but lavishes all its branches on the mastoid and trapezius muscles. To an anatomist it is as plainly set forth as if it were written in our mother tongue, this is *the superior respiratory nerve of the trunk.**

Comparative view of these nerves.

If we examine the *par vagum*, the *portio dura* of the face, the *external thoracic*, the *diaphragmatic*, and the *spinal accessory* nerves, by comparative anatomy, we shall conclude that they are all respiratory nerves, by their accommodating themselves to the form and play of the organs of respiration. In fishes, the respiratory nerve† goes out from the back part of the *medulla oblongata*. When it escapes from the skull it becomes remarkably enlarged, and then disperses its branches to the branchiæ and the stomach. But from the same nerve go off branches to the muscles moving the gills and operculum, whilst a division of the nerve is prolonged under the lateral line of the body to the tail. It is said, this division sends off no branches, but this is not correct; it gives branches in regular succession to the muscles from the shoulder to the tail. Experiments have been made upon these nerves, but their detail would lead us too far. It is scarcely necessary to add, that there is neither phrenic nor spinal accessory, nor

* *Lobstein*, in a dissertation on this nerve, finding the difficulty of accounting for the *nervous fluid* coming by a double passage to the muscle, concludes, *veniet forsan tempus quo istæ quæ nunc latent, dies extrahat et longioris aevi diligentia.*

† The nerve which by its subdivision supplies the heart, lungs, and stomach, and the muscles of the gills,

external thoracic nerves in fishes, the order of their muscular system not requiring them. In birds, the structure of the wing, and the absence of the mastoid muscle, render the spinal accessory nerve unnecessary; it is wanting for the reason that in the absence of the diaphragm there is no phrenic nerve. Quadrupeds have the three respiratory nerves of the trunk; but even in them there are variations in the muscular frame, which illustrate the appropriation of the nerves. The construction of the neck of the camel is like that of birds; there is a succession of short muscles along the side of the neck, and attached to the vertebræ; but there is no long muscle, like the *sterno-cleido-mastoideus*, contributing to the motion of respiration. There is, accordingly, no spinal accessory nerve in the neck of this animal.

We have a remarkable example of the manner in which these nerves vary in their course of distribution, and yet retain their appropriate functions, in the nerves of the neck of birds. In them, the bill precludes the necessity of the portio dura going forward to the nostrils and lips; the nerve turns backwards, and is given to the neck and throat; and it is particularly worthy of remark, that the action of raising the feathers of the neck, as when the game cock is facing his opponent, is taken away by the division of this nerve.

The functions of these nerves farther illustrated.

Before having recourse to experiments on brutes, we may observe what takes place in our own bodies By placing the hand upon the neck, we may be sensible that the mastoid muscle has two motions. The lower extremity of the muscle is fixed when we move the head; but when we use the muscle

in inspiration, the head, and consequently the upper extremity of the muscle, are fixed. Now, if we endeavour to raise the sternum through the operation of this muscle, we shall find that other muscles are, insensibly to us, brought into action, which have nothing to do with this raising of the sternum. For example; if we strain to raise the lower extremity of the muscle, we shall unavoidably produce an action of the muscles of the nostrils; by which association of actions, we shall discover, that we are using the *mastoideus* as a respiratory muscle. If we reverse the action, and move the upper extremity of the muscle, other muscles will be drawn into cooperation, but they will be such as assist in the motion given to the head. Or we may vary the operation in another way. In snuffing or smelling, if we place the fingers on the portions of the mastoid muscles which are attached to the sternum, we shall find every little motion of the nostrils accompanied with corresponding actions of the sternal portions of the muscles in the neck.

A man having a complete hemiplegia, the side of his face relaxed, the arm hanging down powerless, and the leg dragged in walking, we were curious to know if the influence pervaded all the nerves of the side, or only the regular or voluntary nerves. Some trouble was taken to make him heave up the shoulder of the debilitated side, but to no purpose. He could only do it by bending the spine to the other side, and as it were weighing up the paralytic shoulder. But on setting him fairly in front, and asking him to make a full inspiration, both shoulders were elevated at the same time that both the nostrils were in motion. The respiratory nerve of the face, and the superior respiratory nerve, were entire

in their office; and, although the regular system of nerves refused acting, the *sterno-mastoideus* and the *trapezius* partook of their share in the act of respiration. Seeing that the mastoïd muscle has two sets of nerves, that one of these is of the class of voluntary nerves, and the other of respiratory nerves, are we not borne out in concluding, that when the head is moved, being a voluntary act strictly, it is performed through the common class of voluntary nerves? that when the chest is raised, it is an act of respiration, and is affected through those nerves which controul the muscles in respiration?

This conclusion is confirmed by the following experiment. In the ass, there are two muscles which take the office of the mastoid muscle; one is inserted into the jaw, which we may call *sterno-maxillaris*, and the other into the vertebræ, viz. *sterno-vertebralis.* To these the superior respiratory nerve (or spinal accessory) is distributed in its passage to the trapezius. These muscles are at the same time supplied with numerous nerves directly from the spinal marrow. If we expose the superior respiratory nerve, and then induce excited respiration, so as to bring these muscles into powerful action in combination with the other muscles of respiration, and if, while this action is performed, we divide the nerve, the motion ceases, and the muscle remains relaxed until the animal brings it into action as a voluntary muscle.

An ass being thrown, its phrenic nerves were divided, on which a remarkable heaving of the chest took place. It rose higher, and the margins of the chest were more expanded at each inspiration. There was no particular excitement of the muscles of the neck, shoulder, or throat, at this time; so

that to excite the actions of these muscles, it was necessary to compress the nostrils. When they began to act with more violence, keeping time with the actions of the other muscles of respiration, the superior respiratory nerve was divided; immediately the action ceased in the muscles attached to the sternum of the side where the nerve was divided, while the corresponding muscles of the other side continued their actions.

After dividing the spinal marrow between the vertebræ of the neck and those of the back, respiration is continued by the diaphragm: which experiment, as it is often mentioned by physiologists, the author has not thought it necessary to repeat, but only to institute the following experiment on an ass. The phrenic nerves being first divided, and then the spinal marrow cut across at the bottom of the cervical vertebræ, respiration was stopt in the chest; but there continued a catching and strong action at regular intervals in the muscles of the nostrils, face, and side of the neck. The main part of the apparatus of respiration was stopped, but these accessory muscles remained animated, and making ineffectual endeavours to perform the respiration. When apparent death had taken place, the ass was re-animated by artificial breathing, and then these muscles on the face and neck were restored to activity, and became subject to regular and successive contractions, as in excited respiration, whilst the chest remained at rest. These actions continued for a short time, and then ceased; but upon artificial respiration being again produced, the same results followed. This was repeated several times, the animal remaining insensible during these experiments.

Upon stimulating the nerves after the death of this animal, it was observed, that the class of respiratory nerves retained their power of exciting their respective muscles into action, long after the other nerves had ceased to exert any power; they were evidently of that class which retain their life the longest.

It is a duty to avoid the unnecessary repetition of experiments, and I have now to make a short statement of facts, resting on the highest authorities: experiments made without reference to the views now presented to the Society.

The division of the recurrent branch of the *par vagum* destroys the voice.*

The division of the laryngeal branch of the *par vagum* stops the consent of motion between the muscles of the *glottis* and the muscles of the chest.†

The injury or compression of the *par vagum* produces difficulty of breathing.‡

By the assistance of these well known facts, we complete the knowledge of the circle of actions which result from the respiratory nerves.

The *medulla oblongata* and *spinalis* are composed of columns of nervous matter, which (from the different powers of the nerves, as they arise from the one or other of these columns), possess distinct properties. In animals that breathe by ribs and a numerous class of muscles, and which animals have a spinal marrow, we see that a column of nervous

* Sectis ambobus nervis recurrentibus vox perit: *Arnemann, Sömmerring, Morgagni.*

† *Le Gallois.*

‡ Vinculo compressis nervis vagis oriuntur in bestiis spirandi difficultas, surditas, vomitus, corruptio ciborum in ventriculo. *Sömmerring, Haller, Brun de ligaturis nervorum.*

matter is embraced between the anterior and posterior *virgæ* of that body, and that this portion may be traced downwards between the roots of the spinal nerves. From the upper part of this column, where it begins in the *medulla oblongata,* the several nerves proceed which have formed the subject of these papers, and on the influence of which, it has been proved, the motions of respiration principally depend. It is not an extravagant conclusion to say farther, that the power of the regular succession of intercostal and lumbar nerves, as far as they regulate the respiratory actions, proceeds from the connections of the roots of these nerves with this column, which is continued downwards, and which can throughout be distinguished from the rest of the spinal marrow.

We are now enabled to distinguish the influence of the spinal marrow and its regular succession of nerves, from those which have been traced in these papers. The first are essential to the act of respiration; without them the others are unequal to the task. But on the other hand, although the regular succession of spinal nerves be equal to the raising and depressing the thorax, they are not competent to the performance of the motions of the glottis, pharynx, lips, and nostrils, which several parts are necessarily influenced in excited respiration, as well as in the acts of smelling, coughing, sneezing, and speaking: for these, the co-operation of the whole extended class of respiratory nerves is required.

Surveying the complicated machinery which in man is prepared for these various offices, we may reap the benefit of these fatiguing details, in the contemplation of the most interesting phenomena in nature. The relations of the subject

may be presented under the heads of pathology, and expression.

Of pathology, as illustrated by a knowledge of the respiratory system of nerves.

When we survey the full extent of the respiratory system of nerves, we are prepared to comprehend its importance to the continuance of life. The infant born without a brain can breathe if the origins of these nerves be entire. Deep wounds of the brain, though eventually fatal, are not necessarily, or instantly so. The man wounded in the spine below the origins of the nerves which we have traced, drags on existence for a time; but a bruise on the part of the *medulla oblongata,* from which these nerves take their departure, is death in the instant; a breath is not drawn again.

In describing the effects of violence on the medulla oblongata, authors have attributed the sudden death to injury of the roots of the nerves of the par vagum; and yet we have a statement from the same authority, that an animal will survive the division of both nerves of the par vagum. Now that we find that many respiratory nerves depart from the same centre, and go out to all the parts of the muscular frame, which move in respiration, we can better comprehend, how injury of the medulla oblongata suppresses at once the act of respiration in the nostrils, throat, and windpipe, and the action of the muscles both without and within the chest; even the expression of the agony of dying is, by the injury of the roots of all these nerves, suddenly interrupted, and actual death follows quickly, owing to the cessation of the respiratory functions.

A young man was brought into the Middlesex Hospital, who had fallen upon his head. He soon recovered, and lay for some time in the hospital without exhibiting a symptom to raise alarm. He had given thanks to the assembled Governors of the Hospital, and had returned into the ward for his bundle, when, on turning round to bid adieu to the other patients, he fell, and in the instant expired. Upon examining his head, it was found that the margins of the occipital hole had been broken: no doubt it had happened that in turning his head the pieces were displaced, and closed and crushed the medulla oblongata, as it passes from the skull.

A man was trundling a wheel-barrow in Goodge Street, which is immediately adjoining the Middlesex Hospital: in going from the carriage-way to the flag-stones he met the impediment of the curb-stone. He made several efforts to overcome it, and at length drawing back the wheelbarrow, he made a push, and succeeded; but the wheel running forward, he fell, and remained motionless. He was taken into the Hospital, but he was found to be quite dead. The tooth-like process of the second vertebra of the neck had burst from the transverse ligament of the first.* The impulse given to the head had done this violence, and had at the same time carried forward the spinal marrow against the process, and on which it was crushed.

We have seen by experiments, that the respiratory nerves are distinguished from the other nerves by retaining their power longer: that they are alive to impression, and can be

* In my collection there is a preparation which exhibits this ligament destroyed by disease. The death was sudden, and caused by the falling forward of the head, and the crushing of the medulla spinalis.

made to produce convulsions in the muscles they supply, after the other nerves are dead to the application of stimuli. In disease, during the oppression of the mental faculties, and on the approach of death, we witness these nerves, and the muscles put into operation by them, continuing their functions, when in other respects the body is dead. This circumstance, so familiar to the medical observer, might have led to the conclusion to which we have arrived, more laboriously, through anatomical investigations: that there are a great many muscles extended over the body, and which perform the common offices under the will, which are occasionally drawn into combination with the muscles of respiration, and are held in relation to the vital functions by a distinct system of nerves, and that these nerves have a centre and a source of power, different from that of the voluntary nerves.

These nerves, so peculiar in relation and function, are differently influenced by disease from the other division of the nervous system. Their functions are left entire when the voluntary nerves have ceased to act, and they are sometimes strangely disordered, while the mind is entire in all its offices, and the voluntary operations perfect. In tetanus the voluntary nerves are under influence, and the voluntary motions locked up in convulsions; in hydrophobia the respiratory system is affected; and hence the convulsions of the throat, the paroxysms of suffocation, the speechless agony, and the excess of expression in the whole frame, while the voluntary motions are free.

The confusion between vital and voluntary nerves, the combining the par vagum and sympathetic nerves together, and the exclusion of the *portio dura* of the 7th nerve, the spinal accessory nerve, and the external thoracic nerve, from their

natural classification with the diaphragmatic or phrenic, has given rise to very vague theories, and occasioned very inaccurate statements of pathological facts. What remains to be said under this head, I would rather offer in the form of queries.

The frequency of sudden death, where no corresponding appearances are exhibited in the brain or heart, leads us to consider more attentively the only part of the system through which life can be directly extinguished. In *angina pectoris*, we witness the agony of suffering in this system when the patient survives; and when he dies suddenly, we can imagine it to proceed from an influence extending over these nerves, and interrupting the vital operations. We have seen that a branch of this system may suddenly cease to operate on the corresponding muscles, and that in this way the side of the face may be deprived of all participation in the act of respiration, and all expression be lost. What would result from a more universal defect in the actions of this class of nerves, but sudden death?

Could we expect that the diseases of lethargy and somnolency, should be distinctly divided from apoplexies, while the organs on which the distinction of symptoms principally depend, were imperfectly understood?

The stomach, supplied with the great central nerve of this system, exhibits the most powerful influence on these extended nerves; a blow on the stomach "doubles up" the bruiser, and occasions that gasping and crowing which sufficiently indicates the course of the injury; a little more severe, and the blow is instantly fatal.

The position of the asthmatic, shows how this system is

affected; whether directly or indirectly, it is not our present business to enquire. He stands stooping forward, resting his arms so as to throw the muscles of the chest into operation upon the ribs. The position of the head and the rigidity of the muscles of the neck, the action of the mastoid muscle, and of the cutaneous muscle, visible in the retraction of the cheeks and mouth, and the inflation of the nostrils, carry us back in review of the nerves and muscles of respiration.

It will now perhaps be acknowledged, that the methods of physiologists, in accounting for the combination of parts in the actions of respiration, were very imperfect, or rather altogether erroneous. To account for the convulsion of the diaphragm in sneezing, they were constrained to go a far way about: first, connecting the roots of the phrenic with the sympathetic nerve: bestowing sensibility on the latter, which it does not possess: then, following a remote connection between it and the nerves of the nose; then again, counting the relations between the facial nerve and the 3rd of the neck: they satisfied themselves that they had explained the manner in which the diaphragm became convulsed upon irritating the membrane of the nose. Another misconception was engrafted on the first; they spoke of these actions as convulsive and irregular, which are amongst the most admirable provisions for the protection of life. As to the act of sneezing, like coughing, it is a consequence of an irritation of the extremity of one of the respiratory nerves, whence the whole muscles of respiration are brought into action. That there is nothing accidental, nor of the nature of convulsion, is shown, by the admirable adjustment of the muscles to the object. A body irritating the glottis, will call into simultaneous action the

muscles of respiration, so as to throw out the air with a force capable of removing the offending body. But if the irritation be on the membrane of the nose, the stream of air is directed differently, and, by the action of sneezing, the irritating particles are removed from these surfaces. By the consideration of how many little muscles require adjustment to produce this change in the direction of the stream of air, we may know, that the action is instinctive, ordered with the utmost accuracy, and very different from convulsion.

We may notice another office of these nerves; in smiling, laughing, and weeping, the influence is solely propagated through them. The face we have seen is dead to all changes of the kind when the nerve of this class which goes to it is destroyed, whether it be by division of the nerve, or from its being surrounded with inflammation or suppuration. When we consider that all the respiratory nerves depart from the same source, and participate in the same functions; and more especially when we see the respiratory organs so very distinctly affected in the conditions of the mind, which give rise to these affections, it is not too much to suppose, that what is proved in regard to one of these nerves, is true of the whole class, and that they alone are influenced in laughter. Physiologists who have not investigated the cause, are yet agreed in describing laughter to be a condition of the respiratory muscles, where the air is drawn in rapidly, and thrown out in short spasmodic motions of these muscles; that crying is nearly the reverse, the inspiration being cut by spasmodic actions of the muscles of inspiration. By these considerations are explained the *subrisus* which arises from abdominal irritation, and the sardonic retraction of the muscles of the face produced by

wounds of vital parts, and particularly of the diaphragm. It explains also the successive convulsive lifting of the shoulders in wounds of the diaphragm.

That a system of nerves so intimately combined as this is with the other parts of the general system, should suffer in hysterical disorders, cannot surprise us; and admitting that irritation reaches to the respiratory system, we may perceive how rapidly the change may be produced, from the convulsions of laughter to those of crying; and where, if there be a corresponding condition of the mind, it rather follows than precedes the expression of the frame.

These respiratory nerves are organs of expression.

It would have been extraordinary if we had arrived at any satisfactory theory of expression, before it was known through what instruments the mind influenced the body, during emotion or passion. But since we know that the division of the respiratory nerve of the face, deprives an animal of all expression; and that the expressive smile of the human face is lost by an injury of this nerve: since it is equally apparent, that the convulsions of laughter arise from an influence extended over this class of nerves; it comes to be in some sort a duty in pursuing this matter, to examine farther into the subject of expression. We may be at the same time assured of this, that whatever serves to explain the constant and natural operations of the frame, will also exhibit to us the symptoms of disease with more precision.

In terror, we can readily conceive, why a man stands with eyes intently fixed on the object of his fears: the eyebrows elevated, and the eye balls largely uncovered; or why,

with hesitating and bewildered steps, his eyes are rapidly and wildly in search of something. In this we only perceive the intent application of his mind to the objects of his apprehensions, and its direct influence on the outward organs. But when we observe him farther, there is a spasm on his breast: he cannot breathe freely: the chest remains elevated, and his respiration is short and rapid: there is a gasping and convulsive motion of his lips: a tremor on his hollow cheeks: a gulping and catching of his throat: his heart knocks at his ribs, while yet there is no force in the circulation, the lips and cheeks being ashy pale.

It is obvious that there is here a reflected influence in operation. The language and sentiments of every people have pointed to the heart, as the seat of passion, and every individual must have felt its truth. For though the heart be not in the proper sense the seat of passion, it is influenced by the conditions of the mind, and from thence its influence is extended through the respiratory organs, so as to mount to the throat, and lips, and cheeks, and account for every movement in passion, which is not explained by the direct influence of the mind upon the features.

So we shall find, if we attend to the expression of grief, that the same phenomena are presented; and we may catalogue them, as it were, anatomically. Imagine the overwhelming influence of grief—the object in the mind has absorbed the powers of the frame; the body is no more regarded, the spirits have left it; it reclines, and the limbs gravitate, the whole body is nerveless and relaxed, and the person scarcely breathes; so far there is no difficulty in comprehending the effect in the cause. But why, at intervals, is

there a long drawn sigh, why are the neck and throat convulsed, and whence the quivering and swelling of the lip, why the deadly paleness, and the surface earthy cold; or why does convulsion spread over the frame like a paroxysm of suffocation?

To those I address, it is unnecessary to go farther, than to indicate that the nerves treated of in these papers, are the instruments of expression, from the smile upon the infant's cheek to the last agony of life. It is when the strong man is subdued by this mysterious influence of soul on body, and when the passions may be truly said to tear the breast, that we have the most afflicting picture of human frailty, and the most unequivocal proof, that it is the order of functions which we have been considering that is then affected. In the first struggles of the infant to draw breath, in the man recovering from a state of suffocation, and in the agony of passion, when the breast labours from the influence at the heart, the same system of parts is affected, the same nerves, the same muscles, and the symptoms or characters have a strict resemblance.

Having examined the system of nerves and muscles, which are the agents in respiration, in their fullest extent and in all their bearings; having looked at them, in their highest state of complication in the human body, and having traced them upwards, from the animals of simple structure, and then by experiment, and in a manner analytically as well as synthetically, their relations become obvious. Instead of one respiratory nerve, the *par vagum*, the nerve so called, is found to be the central one of a system of nerves of great extent.

Instead of the relations of the vital organs of circulation and respiration depending on some supposed influence of the sympathetic nerve, they are found to have an appropriate system.

This system of nerves, extricated from the seeming confusion in which it lay hitherto encumbered, is found to be superadded to that of mere feeling and agency, attributes common to all animals: through it we see, engrafted as it were, and superadded to the original nature, higher powers of agency, corresponding to our condition of mental superiority: these are not the organs of breathing merely, but of natural and articulate language also, and adapted to the expression of sentiment, in the workings of the countenance and of the breast, that is by signs, as well as by words. So that the breast becomes the organ of the passions, and bears the same relation to the developement of sentiments, as the organs of the senses do to the ideas of sense.

EXPLANATION OF PLATE XXXI.

Fig. 1. Represents the *medulla spinalis.*

A. The *pons Varolii.*

B. B. The anterior medullary columns of the spinal marrow, continued from the *corpora pyramidalia.*

C. *Corpus olivare.*

D. *Corpus restiforme.*

1. The origin of the respiratory nerve of the face.
2. Origin of the glosso-pharyngeal nerve.
3. Origin of the nerve of the par vagum.
4. Origin of the spinal accessory nerve, or superior respiratory nerve of the trunk.

Fig. 2. Plan of the respiratory nerves in their course through the body.

A. *The sterno-cleido-mastoideus muscle.*

B. B. The *trapezius* muscle. It is seen to arise from the back of the head, and from the spine; it is inserted into

C. The scapula, and

D. The clavicle.

E. E. The *serratus magnus anticus.* It is left at its attachment to the ribs, but cut off from its insertion into the scapula, so as to expose the trapezius and the spinal accessory nerve.

F. The lower surface of the diaphragm.

G. The upper surface of the diaphragm.

H. The larynx.

The four great muscles (A. B.B. E.E. F. G.) are associated together, and joined to the organs of the voice, and of smelling, &c. by the nerves displayed here.

To simplify this view, the regular or symmetrical system of nerves is not presented in this drawing, but only the respiratory nerves. It is the entwining of nerves of distinct systems which produces the apparent intricacy. If the spinal nerves were represented crossing these, and the network of the sympathetic superadded to them, we should have all the seeming confusion of the dissected body.

1. Respiratory nerve of the face, or portio dura of authors.
2. The glosso-pharyngeal nerve.
3. The superior respiratory nerve. It is seen to pass through the sterno-cleido-mastoideus muscle, and to supply it with branches: then to take a course

down the side of the neck, branching exclusively to the trapezius muscle.

4. The phrenic or diaphragmatic nerve. It is seen coming out from the spine, and running a direct course to the diaphragm.

5. The external respiratory nerve of the chest. It is like the last nerve in its origin, but it deviates in its course, passes on the outside of the chest to supply the powerful respiratory muscle, the serratus magnus E. E.

These three nerves combine the mastoid and the trapezius muscles, the serratus magnus and the diaphragm, with the larynx, the tongue, and nostrils.

6. 7. The nerve of the par vagum. Coming from the same origin with the other respiratory nerves, it passes down to the internal organs; but in its passage gives off these:

8. The superior laryngeal nerve, a branch of the last nerve.

9. The recurrent nerve; a branch also of the nerve of the par vagum. Where the nerve of the par vagum is in the thorax (7) at the same time that it sends off the recurrent (9), it sends off many small nerves to the heart and the lungs, and then descends in a plexus on the œsophagus, to the stomach.

XXIV. *Experiments and observations on the Newry pitch-stone, and its products, and on the formation of pumice. By the Right Honourable* George Knox, *F. R. S.*

Read May 9, 1822.

The locality of this mineral, and the singularity of its external characters, having excited my curiosity, I took advantage of the facilities furnished by the liberality of the Royal Society of Dublin, of which I have the honour of being one of the Vice Presidents, to make the subjoined analysis in their laboratory.

Doctor Fitton, in an excellent paper inserted in the first volume of the Transactions of the Geological Society, has given a minute description of the site and external characters of the Newry pitch-stone. I shall transcribe it, previous to laying before the Royal Society my own observations. I do so with more satisfaction, as we in general agree; in fact, scarcely two specimens are exactly the same, although contiguous to each other in the vein; some being compact, some thin slaty, some olive, and some leek-green; some so disintegrated, particularly when exposed to the air, as to be friable between the fingers, while others retain their gloss, consistency, and colour, with much tenacity, although they all fall at length into rhomboidal fragments. I may add also, that while some are quite porphyritic, others have but a few specks of felspar on their surface.

However, Dr. FITTON seems to have overlooked two very striking characters of it, which engaged my particular attention, and gave birth to those conjectures, which principally induced me to undertake the analysis by which they have been verified. I mean the *smell*, and strong oily taste.

"This substance (Dr. FITTON observes), is found in a "vein traversing granite, in the vicinity of Newry, in the "County Down. Its colour is intermediate between moun-"tain and leek-green. It is massive. Fracture small, and "not very perfect conchoidal.

"Internal lustre, resino-vitreous, and shining. It exhibits "lamellar distinct concretions; the plates are from one-"fourth to one-tenth of an inch in thickness, and are further "divisible into pieces of the rhomboidal form of various "angles. The surface of the concretions is smooth and "strongly glistening. Slightly translucent on the edges. It "scratches window glass, but is easily scratched by quartz. "Easily broken. Specific gravity 2,29. Before the blow-"pipe, without addition, it yields a greyish-white frothy "enamel. It is in some places porphyritic, containing im-"bedded minute crystals of felspar and of quartz.

"A letter from a very intelligent observer, who has ex-"amined this substance in its native place, states the follow-"ing particulars respecting its position:

"The vein is first observable in the Townland of Newry, "at the bottom of a bank of granite, about half a mile from "the northern end of the town, on the right of the road lead-"ing to Downpatrick. It crosses the road, and runs due "westward, ending on the side of the great road from Newry

" to Belfast. Its length, so far as hitherto observed, is half " a mile.

" The rock, which is covered with mould to the depth of a " foot, consists of a grey granite. The vein is about two feet " and a half, or two and a quarter in width; at the places " of contact, both the granite and pitch-stone are disinte- " grated, the latter being almost as soft as clay, but becoming " gradually harder as it approaches the center of the vein. " The structure of the vein is foliated, the folia being per- " pendicular to the horizon, and also to the walls; and, be- " sides these, there are seams, that run longitudinally, parallel " to the horizon, and nearly perpendicular to the folia.

" Although this substance presents some peculiarity, in " being divisible into rhomboidal fragments, it approaches in " this respect to the *pitch-stone* of Arran, (in lamellar concre- " tions) which holds as it were a middle place between it, and " that possessing the more usual characters.

" Mr. JAMESON has described a vein of pitch-stone " running " in granite" observed by himself in Arran;* and he states " that lamellar distinct concretions have been hitherto ob- " served in the pitch-stone of that island only."†

To the geological account of this mineral, above given, I have only to add, that the granite which it traverses is rather of a soft kind, containing much felspar, and disposed to be converted, by decomposition, into lithomarge; and that a vein of basalt nearly parallel to it, at the distance of about an hundred yards, passes through the same rock of granite. This basalt very readily disintegrates by exposure to air and moisture; and it contains spheroidal concentric balls of basalt of

* Min. of Scottish Isles, 4to. vol. I. p. 81. † JAMESON's Mineralogy, vol. I. p. 261.

a loose contexture; and likewise a considerable quantity of compact stilbite, approaching to fibrous, and of a schistose structure.

I obtained specimens of the pitch-stone of various kinds, compact, thin slaty, and disintegrated; and one of the thin slaty was of a different colour from the rest, darker, more porphyritic, and of a more shining lustre.

The external characters of all agreed in the following particulars.

Specific gravity 2,31. Fracture, small conchoidal; fragments indeterminately angular, rather sharp edged and rhomboidal.—Surface, smooth and glistening.—Feel, unctuous.—Lustre, resinous.—Hardness, semi-hard, the sharp edges scratching glass.—Streak and powder whitish grey, passing into greenish grey.—Smell, *oily*.—Before the blow-pipe, fuses without addition into a pale leek-green glass. Colour of the most porphyritic specimen, perfect leek-green; of the rest, olive-green, passing into oil-green.

Although the peculiar character of this variety of pitch-stone is its smell, yet, I believe, it differs from all others, including those from Arran, in the *degree* in which it is disposed to divide into thin laminæ; its proneness to disintegrate, and the regularity of its rhomboidal fragments. So much is it inclined to disintegrate, that a piece, nearly compact, after lying for some days on the table of a warm laboratory, fell into large rhomboidal fragments.

It will appear, I think, that these qualities proceed from the same cause.

A piece of the compact specimen was exposed for half an hour to a red heat in an open crucible. It lost 7,75 per cent.

of its weight. The colour changed to a pitch brown. It retained its lustre. It opened, in the manner of cannel coal, into thin slaty fragments, but without actually falling in pieces, and the interstices acquired a more greasy lustre, and smoother appearance, than the rest of the stone.

The circumstances above detailed, namely, the *oily smell*, the disintegration, and the effect of ignition, excited a suspicion that the mineral contained some inflammable substance.

One hundred grains of the compact, in fine powder, were exposed to a white heat in a platina crucible, and were converted into a glass of a very pale leek-green colour, and lost ten per cent.

Two hundred and twenty grains of the same, coarsely powdered, were put into a small coated (common bottle glass) retort, to the mouth of which a phial, previously weighed, was luted with white lute, and exposed for half an hour to a red heat. A colourless liquor came over, which had a slightly bituminous smell. The receiver had gained 16 grains, being 7,2727 per cent.

The retort breaking, I was not able to ascertain the loss of weight of its charge.

The stone finely powdered, and projected on melted nitre, scintillated a little.

I now proceeded to my analysis, in the usual way, following the method of KLAPROTH, in his analysis of the pitch-stone from Meissen; my view having been, at first, rather to ascertain ingredients, than proportions.

It may be as well to mention, in this place, the result of KLAPROTH's analysis. It was

Silex	73
Alumina	14,50
Lime	1,00
Oxide of iron	1,00
Oxide of manganese	0,10
Soda	1,75
Water	8,50
	99,85

He obtained his silex by exposing 100 grains of the pulverised stone, along with 200 grains of caustic soda, to a strong red heat, for half an hour, in a silver crucible; when cool, softening with water, dissolving in muriatic acid, evaporating nearly to dryness, diffusing in water, and filtering.

The alumina was obtained by boiling the muriated solution, thus freed from silex, with excess of caustic soda. The alumina was held in solution while the lime and iron were thrown down. The alkali was then neutralised, and the earth precipitated by carbonate of soda. The precipitated lime and iron, first dissolved in muriatic acid, and then combined with sulphuric acid, gave sulphate of lime, which was collected and washed with weak spirits of wine, and the pure lime was estimated as in the proportion of one to three in the sulphate.

The iron was precipitated by carbonate of ammonia. Some flocks, which appeared in the remaining fluid after it had been evaporated to dryness and re-dissolved, were *presumed* to be manganese, and *estimated* at one-tenth of a grain.

I proceeded in the same manner but obtained no manga-

nese; and in the liquor from which the sulphate of lime was extracted, there was no sulphate of magnesia.

The alumina boiled in sulphuric acid and mixed with sulphate of potash was converted into alum, leaving nothing in solution.

The first thing which caught my attention in the course of this process, was the appearance of a black insoluble matter in the muriatic solution from which the silica had been separated. As it evaporated with considerable rapidity, in consequence of the great heat of the sand bath, a rim was left from time to time on the surface of the pan of dry salt, which, from being orange, became white, and shortly after a black powder used to separate from it, fall to the bottom of the pan, and remain undissolved.

This I at first suspected to be oxide of manganese; but having collected it, I found that it was insoluble in acids, and that it burned away at a red heat.

I then conceived that some carbon had accidentally got into the liquor; but as the same phenomena recurred with every experiment, and I repeated my analysis many times, I became convinced that it belonged to the stone. Other circumstances, however, put it out of doubt.

To obtain the alkalies, if any existed in the stone, I pursued each of the methods usually recommended; that invented by ROSE, and practised by KLAPROTH; that of Sir HUMPHRY DAVY; the new process by CERUSE; and the action of acids on the stone; by the last of which I finally abided.

KLAPROTH mixed 100 grains of the pulverised stone with 300 of nitrate of barytes, ignited them in a porcelain crucible, and neutralised the mass with muriatic acid, which he

expelled again with sulphuric acid. The inspissated mass he washed with hot water, filtered, and saturated the filtered liquor with excess of ammonia. The residuum being separated by the filter, the remaining fluid was evaporated to dryness; the sulphate of ammonia having been driven off at a moderate heat, sulphate of soda was left behind. This being dissolved, was decomposed by acetate of barytes, and filtered; the filtered solution evaporated to dryness, and the dry salt exposed to a red heat in a platina crucible. The saline residue was then re-dissolved and evaporated to dryness. It was carbonate of soda.

I have detailed this well known process, chiefly to remark an occurrence in one part of it, which continued to confirm me in the opinion that the stone contained an inflammable substance.

When I was expelling the sulphate of ammonia, I was struck by the melted mass in the crucible, which had before been white, suddenly becoming black, and, as the sulphate of ammonia flew off, gathering into a black powder. This powder being insoluble in water, was easily collected; and, like that which I had got before from the acid solution, resisted the acid, and was volatilised by a red heat.

The alkali which I obtained was soda. By converting it into nitrate, sulphate, and muriate, and observing the crystallization of each, and by testing it with muriate of platina, I ascertained that it contained no potash. It had none of the characters of lithia, neither the crystals of the muriate nor of the nitrate were deliquescent, nor did they tinge the flame when burned with alcohol. When a small portion of the stone, enveloped in soda and placed on platina leaf, was treated by

the blow-pipe, it left no black mark on the metal: it contained therefore no lithia.

I proceed now to my final analysis, premising that, although the last, I think it may be pretty well relied upon. I had made a separate one for each ingredient, giving my whole attention to that which I had selected, without embarrassing myself much with the others.

In this final analysis I adopted a different process, considering it more convenient. My compact pitch-stone being exhausted, I was obliged to resort to the next most compact, and which I call the *slaty compact.*

One hundred grains, mixed with 200 of caustic soda in a silver crucible, were exposed to a red heat. When cool, the contents of the crucible were softened with water and dissolved in muriatic acid. A jelly was formed, by evaporating the solution slowly on a sand bath. When nearly dry, it was diffused in water and filtered. The silica caught on the filter, and exposed to a red heat, weighed 70,8 grains. (*a*)

The filtered liquor (*a*) was evaporated to dryness, and the mass re-dissolved; there remained a powder, which dried at a red heat, weighed 0,75, and was silica. (*b*)

The liquor (*b*) from which the 0,75 of silica had been separated, was precipitated by ammonia and filtered; alumina and iron remained on the filter; the lime went through in solution (*c*).

The iron and alumina were washed completely off the filter into a silver evaporating dish before they were dry, by projecting against it water, impelled by the breath through a dropping glass; a mode of removing substances from filters always effectual, and far preferable to drying and collecting

the dry powders from the paper. It was suggested by Mr Wharmley, Chemical Assistant in the Laboratory of the Royal Society of Dublin.

The alumina and iron thus collected, were boiled in caustic potash until a separation had taken place, the iron remaining as a powder at the bottom of the silver pan, and the alumina being held in solution by the alkali *(d)*.

The iron separated by the filter in the manner above mentioned, was dried and weighed. It weighed 5,25 grains. It was then dissolved in muriatic acid, with which it effervesced, and therefore had been carbonated. A powder remained behind weighing 0,3 grain, which was silica. The iron was then precipitated by caustic potash, filtered, and again evaporated to dryness; it weighed 3,4 grains, which gives of protoxide of iron 3,036 grains.

The solution *(c)* evaporated to dryness and re-dissolved, left a residuum which weighed one grain, and which being insoluble in boiling sulphuric acid, and being perfectly white and gritty, I concluded to be silica. It was then precipitated by carbonate of soda, filtered, washed and dried; the precipitate, which was carbonate of lime, weighed 2 grains. Now, 2 grains of carbonate of lime may be estimated at 1,12 of lime.

The alkaline solution *(d)* was precipitated while hot by muriate of ammonia, filtered, washed, and ignited in a platina crucible; it weighed 11,50 grains.

Extraction of the alkali by acid.

Nitric acid.

100 grains of the pulverised stone were boiled with dilute

nitric acid. When dry, the soluble part was taken up by water and replaced by fresh acid. This process was repeated until the acid seemed to have no farther effect. The aqueous solutions were then thrown together and evaporated to dryness. Alcohol removed the nitrate of lime; and the iron being peroxidated by the nitric acid, nothing remained when the mass was re-dissolved in water but nitrate of soda, which crystallized in cubes, and had every other property of nitre. This being evaporated again to dryness, and weighed, was found to be 7,75 grains of nitrate of soda, which gives 2,857 for soda. I consider this to be the real proportion of the alkali. It differs but little from the following result by

Muriatic acid.

100 grains were treated in the same way with muriatic acid, the iron being peroxidised when the aqueous solution was evaporated to dryness. The chloride of sodium weighed 5 grains. Now, 5 grains of chloride of sodium make of dry soda, or oxide of sodium, 1,98198, being in the proportion 55,5 to 22; but 1,98198 of dry soda produce 2,87044 of hydrate of soda, the state in which it is probable the alkali exists in the stone.

In the process with the muriatic acid two new and decisive phenomena presented themselves.

When evaporating off the excess of muriatic acid from the solution in a platina crucible with a silver cover, I observed the inside of the cover, which I had laid lightly on and removed from time to time, to be coated with a yellow substance, which had a bituminous smell. I placed the cover with the

top down on a chafing dish of charcoal, and in a few minutes the whole was volatilised, and the silver became bright.

On evaporating the alcohol, in order to ascertain how much lime the acid had taken up, I observed a dark oily substance collecting as the alcohol diminished, and which, when the solution was nearly dry, was collected, and weighed 3,70 grains. It had an empyreumatic smell, was insoluble in ether, but dissolved in spirits of turpentine, and inflamed with difficulty with a thick smoke and pungent odour. Naptha dissolved it only in part, and changed the colour to grass-green. It seemed to retain some muriatic acid, and gave indications of containing some iron; it was therefore impure bitumen.

All these circumstances combined to urge me to obtain the bituminous matter of the stone, if possible, in a state of purity, and to ascertain the quantity which existed in it.

I therefore engaged in the following experiments:

I procured an iron retort about 6 inches long and half an inch in diameter, one end of which was screwed to a bent gun-barrel, and the other, or bottom, closed by an iron screw. This apparatus was perfectly well made, and air tight.

Experiment 1.

Into the retort I put 480 grains, in coarse powder, of the dark leek-green thin slaty Newry pitch-stone, as, from its appearance, I judged that that variety contained the greatest proportion of bitumen.

The retort was then enclosed in a long earthen crucible, the interstices being filled up with glass-house sand, and placed in a powerful side furnace. To the end of the gun-

barrel was affixed, and luted with white lute, a glass tube, with a hollow ball near its junction with the mouth of the retort, for receiving liquids, and fitted at the other to a mercurial trough, in order to collect the gases that might come over.*

A considerable quantity of gas came over, in addition to the air of the vessel, and when the retort had acquired a red heat, a liquor distilled which was evidently water.

When the heat was urged farther, another liquor made its appearance. It was slightly coloured and ran down, in oily streaks, into the receiver, where it floated on the water.

When the distillation had ceased the apparatus was removed, all access of air excluded from the receiver, and the gases examined.

They consisted of carbonic acid, which was removed by lime water and caustic potash; of hydrogen, which was judged of by its inflammation; and of carburetted hydrogen, which was tested by adding to a portion of the gas, deprived of carbonic acid, some oxygen gas and exploding by electricity. Carbonic acid was thereby generated, as was proved by its rendering lime water turbid.

The receiver, which had been previously weighed, was now examined. It had gained 7,81 grains per cent.

The oily fluid had the smell of tobacco, exactly that of a tobacco-pipe long in use, and it burned with a similar flame to naptha: while burning it had the same smell as petroleum, which I had collected myself at the Pays de la Poix, near Clermont, in Auvergne. The water was neither acid nor alkaline.

* When I ceased to look for gas, I discontinued the luting.

The experiment was repeated, but without an attempt to catch the gases; and the moment the smell came over the receiver was changed, by which means nearly the whole was obtained, independent of what it suffered from decomposition, and weighed about 2,5 grains per cent.

Experiment 2.

Finding that the iron decomposed the water, and thereby in part the bitumen also, I gave up that apparatus, and had a very strong glass retort, such as is used in the manufacture of common wine bottles, made of a similar shape, and coated, to which a receiver of the same form as those described was luted as before.

I may mention here a very curious appearance of the mass which remained in the iron retort. It was of course of a cylindrical shape; its colour was pale ash-grey; it was porous, semivitrified, and slightly cohering; resembling coarse pumice, and floated on water. But the circumstance to which I now allude is that it separated, when broken, into thin regular tables, precisely in the manner of madreporites, so that it might be considered as schistose pumice.

In this experiment, I gave to 480 grains of the coarsely powdered leek-green variety, a dull red heat for half an hour, in order to expel the water, as I had found that, with that degree of heat, the bitumen was not removed from the stone. I then charged it into the glass retort, inserted in a crucible as before, and gave it a white heat. No gas was generated, nor expelled, and the liquor in the receiver was pure bitumen. It weighed 2,83 grains per cent. and was of a wine-yellow colour, and the smell the same as the last. The neck of the

retort being thinner than the rest had fused, but fortunately not until the whole of the bitumen had come over. The residuum was again a pumice, exactly the same as in the experiments with the iron retort.

Experiment 3.

Slaty compact pitch-stone.

A green glass retort was again charged with the pitch-stone which I had been analysing, namely, the slaty compact; but the neck of the retort, being thinner than the body, fused. On examining the mass within the retort, a light-coloured pumice was found at the bottom, and a dark one at the top.

Experiment 4.

100 grains of the slaty compact pitch-stone which I had been analysing, lost by ignition 8,0 per cent.: when fused it lost 0,5 more, and formed a light ash-grey glass.

Experiment 5.

480 grains of the same were distilled after the water was expelled by ignition, and therefore the danger of decomposition removed, in an iron retort. Bitumen came over, and the process appearing to be completed the receiver was removed, after which some more bitumen dropped from the retort. The bitumen in the receiver smelt more of naptha than the former products, but that which dropped had the same smell of tobacco. What was condensed in the receiver was very volatile, being driven from one end of the phial to the other, namely, by the heat of the hand. The contents of the retort were a light pumice, as before.

Experiment 6.

400 grains of disintegrated Newry pitch-stone which had lost 7,25 per cent. by a red heat, were distilled as usual; 0,1 came over of bitumen, which had the smell and volatility of naptha.

Experiment 7.

100 grains in mass of Meissen *pitch-stone* (the same as that which had been analysed by KLAPROTH, and for which I was indebted to my friend Sir CHARLES GIESECKE, Professor of Mineralogy to the Royal Society of Dublin) were ignited in a platina crucible. The colour changed from oil-green to greenish white passing into greyish green; it opened in the same manner as the compact Newry pitch-stone, showing a tendency to slate, the fissures being parallel; fragments indeterminately angular and rhomboidal, and the lustre altered from resinous to waxy. It was afterwards fused into a white enamel and lost 8 per cent. As KLAPROTH had lost 8,50 by simple ignition, I concluded, erroneously, that it contained no bitumen.

Experiment 8.

400 grains of pitch-stone from Meissen were distilled as usual after ignition: a small quantity of bitumen came over which had the smell of naptha, and was more volatile than any of the former products. The contents of the retort were fused into a greyish white enamel.

Experiment 9.

Having ignited 400 grains of Meissen pitch-stone, and afterwards distilled them in the usual manner, the receiver having

been previously very accurately weighed, 0,5 grain of bitumen were obtained, equally as volatile as that procured in the last experiment. The retort not having received so strong a heat as in the former experiments, only a slightly cohering perfectly white powder remained.

Experiment 10.

On Arran pitch-stone.

I fused 100 grains of pitch-stone from the Island of Arran, in a platina crucible at a white heat; it formed an ash-coloured glass, and lost 5 per cent.

400 grains of the same stone were charged into a green glass retort, as in experiment No. 2. Water came over, and when the heat was increased, was followed by an oily substance; but at the same moment the neck of the retort fused, as in experiment 3, and the distillation ceased. The receiver had acquired an additional weight of 16 grains, that is 4 per cent. It had the smell of the bitumen from the Newry pitch-stone, but faint, and the quantity was too small to be separated. On examining the fused retort, it appeared that, for the distance of an inch from the bottom, the colour of the stone had become, as in former experiments, of an ash-grey, and still resembling pumice, but that the upper part was brown, as in experiment 3; so that it seemed as if the bitumen had risen from the more strongly heated part of the retort, and having been stopped in its progress by the fusing of the neck, had given to the upper part of the mass the same brown colour which I had observed when the whole stone had been originally ignited in an open crucible, as mentioned in the beginning of this paper.

100 grains of the brown part of the residuum of this pitch-stone were then fused, as before, in a platina crucible, into a light-grey glass; it lost one per cent.; thus making up the 5 per cent. which had been the loss from the original fusion.

Experiment 11.

400 grains of Arran pitch-stone lost by ignition 4,5 per cent.; and when distilled in the usual manner in an iron retort, one grain was collected in the receiver, possessing a very peculiar smell. The contents of the retort were a perfect enamel. The receiver was washed with sulphuric ether, which was suffered to evaporate, and a substance remained on the glass pan, but too small in quantity to enable me to examine it.

Experiment 12.

Artificial pumice.

100 grains of artificial pumice, which had been the result of my first and second experiment, were fused in a platina crucible into a greyish-white glass. It lost nothing.

OBSERVATIONS.

I shall, for the sake of distinctness, divide my remarks under heads, beginning with the analysis.

Analysis.

Having mistaken the bitumen which appeared so frequently in the course of all my analyses of the stone, for manganese, and having found, I think, that inflammable substance* in the

* Vide Experiment.

Meissen pitch-stone, will it be presumption to suspect that Klaproth made a similar mistake; and that the substance, which he does not appear to have examined, and which he acknowledges he did not weigh, was, in reality, bitumen? This cannot affect the character of this admirable analyst, since there was nothing in the appearance or external characters of the Saxon mineral which could awaken a doubt on that subject; whereas, the peculiar properties of the Newry pitch-stone, naturally, and early, arrested my attention, and would equally have engaged his, had it been submitted to him.

Klaproth's assertion, however, that his pitch-stone contained manganese, induced me to institute many experiments to obtain the same substance from mine, and in the course of them I had occasion to try the different methods of separating manganese from iron, recommended by Hatchett and Herschel.

I mixed perfectly pure oxides of each metal in ascertained proportions, and dissolved them together in muriatic acid, and I found they could be perfectly separated by both methods. I had, at first, some doubts as to Herschel's, but found that my failure had arisen from neglecting to stir the liquor during the precipitation of the iron. In general, I think, the separation is more easy by Hatchett's mode, but, under some circumstances, Herschel's will be found preferable.

There is one method of obtaining, in an analysis of a stone, the iron combined with the silica alone, and as muriatic acid easily separates them, it may sometimes be found useful. The heat which decomposes muriate of iron leaving an insoluble oxide, does not decompose the earthy muriates; therefore,

if the mass be dried on the sand bath at a heat below redness, when it is afterwards diffused in water, the salts will be dissolved, and the iron and silica will be obtained on the filter.

With respect to the alkalies, the fusion with nitrate of barytes is a tedious process, and liable to loss from the frequent filtrations, and also from the necessity of evaporating off the sulphate of ammonia.

That of Sir HUMPHRY DAVY, by boracic acid, is much neater; but, probably from want of analytical skill, I found it so difficult to get rid of the boracic acid, that I was not able to rely upon my result.

The new method, by lead, has its embarrassments also, particularly from the fusibility of the glass which acts upon platina, and penetrates clay crucibles. There is also a good deal of difficulty in getting entirely rid of the lead.

The process by acids, where the nature of the stone will admit of it, is obviously the best; and I found that the nitric acid was to be preferred, as, by its peroxidizing the iron, and destroying the bitumen, those two troublesome substances were disposed of.

Bitumen.

It has appeared, from the foregoing analysis, that this substance may be obtained both in the usual mode of analysing, and by distillation of the stone, but quite pure only in the latter mode. It seems to consist of two inflammable substances, the one much more volatile than the other, but both inseparable from the stone, except at a heat approaching, if not entirely amounting to, whiteness. I imagine that it is in combination with the iron, as it seemed in general to accom-

pany the solutions of that substance, and to modify the colour and magnetic properties of the metal.

It is necessary to observe that, unless the beak of the retort be kept almost at a red heat, as far as the mouth of the receiver, the whole of the bitumen will not be obtained.

This quality renders it easy, however, to ascertain the proportion in which it exists in the stone; a heat of ignition expelling only the water, and the loss of weight by fusing after ignition, giving pretty exactly the quantity of this curious inflammable matter, which appears to vary considerably in the different specimens from the same vein.

I have not yet been able to examine it with sufficient accuracy, partly from want of time, and partly from the difficulty of analysing so small a portion of the substance as I have yet been able to collect. I hope, however, to renew the investigation next winter. If it should be found to be a new substance, I propose to call it *newrine*. I should not be surprised, however, judging from the smell, and its being separable from water by evaporation, if it were found to contain *nicotine* in combination with naptha.

Pumice.

The formation of this substance *artificially*, is rather an interesting circumstance, if it be new; and may throw some light upon its natural formation.

That it is a perfect pumice, in appearance and qualities, what I have already mentioned puts almost out of doubt. It has the colour, levity, and magnetic properties of the real pumice, and deceived artists to whom I presented it as such. An eminent paper stainer, who is in the constant use of that

article, and to whom I sent two pieces, desiring to know, from him, which of the two specimens was the best; answered, that both were excellent, but that the lighter was the best. He had no suspicion that they were an artificial product. If the analysis of pitch-stone and pumice be considered together, it will be found that the water, bitumen, and lime, in the former, are the substances which alter the per centage.

How the lime was got rid of by the volcanic heat is not easily accounted for; but it is conceivable that it may have been acted on by the muriatic acid fumes, which, we know, accompany volcanic fires. Besides, lime may yet be found in some pumice, and none of that earth in some pitch-stones. As yet, I believe, there have been but two pitch-stones, the Meissen and the Newry, analysed, and the former, only, by a chemist of authority. It must be acknowledged, however, that it is difficult to imagine, on this hypothesis, how the alkali remained un-muriated, while the lime united to the muriatic acid.

It appears to be a condition, in converting a stone into pumice, that it should contain a volatile substance, which can only be removed by the same degree of heat which is at the same time necessary for producing that sort of semi-vitrification in the mass which renders it coherent, hard, and porous. If a stone contains only water, pumice is not formed, because that is driven off by a red heat, which does not act upon the earths of which the stone is composed. Drive off the water at a red heat, and you have a harder but incoherent mass: increase the heat, and you have, as in the case of alumine, a more compact and denser substance, or else, when the ingredients of the stone favour vitrification, a glass.

I have made pumice in an open crucible, and with the unpowdered stone, by watching the degree of heat.

Magnetic properties.

A needle, drawn a little out of the direction of the poles by a piece of iron, was drawn back by all the pitch-stones, and by pumice, natural and artificial.

The pumices, as well the natural as the artificial, and the latter, whether obtained by distillation in glass or iron, were attracted, when in powder, by the magnet; but the magnet did not act on any of the pitch-stones under any circumstances.

RESULT.

If the foregoing analysis is correct, that variety of the Newry pitch-stone which I examined contains in one hundred parts,

Silica	72,800
Alumine . . .	11,500
Lime . . .	1, 12
Protoxide of iron .	3,036
Soda . . .	2,857
Water and bitumen	8,500
	99,813

The *Newry* pitch-stone is a *marked variety* of that substance.

It exists in various forms, and the bitumen in various proportions in the same vein.

It contains, as far as at present appears, a peculiar bitumen.

That bitumen exists in the Newry pitch-stone in a larger proportion than in any substance of that class hitherto examined.

The darker specimens appear to contain most bitumen, nearly 3 per cent.

The bitumen, as well as the soda, can be separated from the stone by the action of nitric and muriatic acid, but the bitumen not in a state of purity.

The bitumen can be obtained pure by distillation from the stone by means of a heat nearly approaching to whiteness.

It can be separated from water by evaporation. The Newry pitch-stone is converted into pumice when properly distilled.

It is magnetic, but not so much so as its pumice.

It is not electric, either by heat or friction, neither is its pumice.

It probably owes its *smell, slatiness,* and disposition to *disintegrate,* to the bitumen.

The bitumen probably modifies its *colour* also.

The Newry stone agrees, as to its ingredients, with the Saxon pitch-stone from Meissen; but they differ somewhat in their relative proportions, and particularly in the quantity of bituminous matter.

The Arran pitch-stone seems also to contain bitumen; but on this point I cannot yet form a decided opinion.

I had begun a course of experiments on other substances, namely, green-stone, basalt, obsidian, &c., but I have been obliged to discontinue them for the present. I cannot however abstain from mentioning one experiment which failed, as it was attended by a circumstance which gives it some interest.

I ignited 400 grains of green-stone, which lost 5 grains; and charged it into an iron retort as usual. The receiver increased 0,2 in weight, and there was no smell. On examining the contents of the retort after the distillation, I found that the powdered stone had been converted at the bottom into a vesicular glass, and at the top into a pumice, but that a hole had opened in the upper part of the neck of the retort. It seemed therefore that, in the first place, most of the volatile matter must have escaped, whatever that volatile substance may have been, through the hole, but that the temperature having been lowered in its vicinity by the admission of air, the formation of glass was checked as far as that influence extended, and the result was pumice. Why the glass was vesicular and not compact, I shall leave to conjecture. It is probable that farther experiments may elucidate that point.

NOTE.

To assist in forming a judgment whether manganese really existed or not in the Meissen pitch-stone, to 100 grains of the pulverised stone was added 0,1 of oxide of manganese, and fused. The colour was whitish, as before, but as the great heat necessary to fuse the substance might have discoloured the manganese, the following experiments were made. To 99,9 grains of pure white glass, 0,1 of black oxide of manganese was added. It fused into a violet-coloured glass; but when the heat was increased the colour was removed. To try, therefore, whether a violet glass might be produced from a fusion at a lower temperature of

the Meissen pitch-stone, it was first fused with borax by the blow-pipe: no trace of a violet-colour appeared, but, when afterwards fused by the blow-pipe with 0,1 addition of black oxide of manganese, the colour produced was a violet.

XXV. *Observations on the changes the egg undergoes during incubation in the common fowl, illustrated by microscopical drawings.* *By Sir* EVERARD HOME, *Bart. V. P. R. S.*

Read May 16, 1822.

THIS has been a favourite subject with many of the most celebrated anatomists. The great HARVEY, MALPIGHI, and JOHN HUNTER, have done more than any others in the advancement of this enquiry.

HUNTER'S observations were never rendered so complete as to induce him to publish them; but what he has done is open to the public, both in the preparations in his Collection, and in his Cabinet of Drawings.

In these last, the vesicle which begins near the rectum, and afterwards envelopes the embryo, is clearly made out.

In the year 1815, Mr. DUTROCHET, an ingenious French anatomist, laid a Memoire upon this subject before the French Academie. Mr. G. CUVIER has given a Report upon this Memoire, but has not given the Memoire itself. He considers the author to be the first person who has clearly explained the rise and progress of the vesicle; but some of Mr. HUNTER'S drawings of it are dated 1773, the year I went to live in his house.

After all that has been done, much is still wanting to render the investigation complete; and to promote this object, the following Observations and Drawings are brought before the Society.

The gelatinous molecule, from which the future embryo

is to be formed, is originally placed on the surface of the yelk; it is found there before the yelk leaves the ovarium, and lies loose upon it, not being enveloped in any capsule.

The external membrane of the yelk is very thin and delicate, its surface is studded over with red dots, which disappear in its passage along the oviduct. When this is removed, there is a second thick and spongy covering under it, in which there is a natural aperture; and the areola, surrounding the molecule, is nothing more than the surface of the yelk, that is circumscribed by the margin of this aperture. No such aperture has been before taken notice of.

The molecule itself has a granulated appearance; in the centre it is made up of globules $\frac{1}{2800}$ part of an inch in diameter, surrounded by circles of a mixed substance, consisting of about two-thirds of the same small globules, and one-third of larger oval globules, about $\frac{1}{1600}$ part of an inch in length, and $\frac{1}{2000}$ part in diameter; these last, in their figure, resemble the red globules of the blood in the bird in every respect, excepting their red colour. Besides the globules there is some fine oil, which appears in drops when the parts are immersed in water. Oval globules and oil are also met with in the yelk itself, but in small proportion, and without colour.

All these parts, except the red dots on the surface, are met with in the yelk on a smaller scale, even six days before it is completely formed.

The ovarial yelk-bag gives way at the middle line, farthest from the insertion of the blood-vessels, and the yelk drops out into the mouth of the oviduct.

The yelk-bag does not immediately close, although it con-

tracts considerably: some time after it is nearly obliterated; and on the pedicle, the rudiments of a new yelk are formed.

The yelk-bags are exceedingly vascular; the outer membrane of the yelks is connected to them by vessels and fasciculi of fibres, but easily separated.

The yelk, while in the ovarium, has an oval form, and lies with its long axis towards the pedicle of the bag. See Plate XXXII. figures 1, 2, 3, 4, 5.

In its passage along the oviduct the yelk acquires the albumen, and before it comes to the lower end, the albumen is covered by a very fine membrane.

In this passage the thread-like substances, called by Mr. Hunter the poles, by others the chalazes, are formed; and terminate in the double membrane, which is added at the time the egg has reached the enlargement at the lower end of the oviduct. In the cloacus the shell is formed. If the egg is taken out before it has acquired a shell, it remains soft for several days; in one instance, after four days, it was so semi-transparent as to receive a yellow tint from the yelk. On puncturing the covering, the contents rushed out, but immediately resumed their form, being inclosed in the thin membrane of the albumen. The molecule with its areola, and the chalazes, were distinctly seen. The whole contents had less volume than the shell, particularly in the long axis, being truncate at both extremities. When immersed for an hour and half in distilled vinegar, the albumen and other parts had become somewhat coagulated. Plate XXXII. fig. 6, 7, 8, 9.

In the new laid egg the appearances are exactly the same,

whether it is impregnated or not. Plate XXXII. fig. 10, 11, 12.

When the shell and membranes under it are removed from one side, the yelk appears to be kept in its place by the poles, although allowed to rotate upon its axis.

The gelatinous molecule, with its areola, is always found upon the highest point of the upper surface of the yelk. Whether this arises from the molecule, or from the areola being the lightest part of the yelk, has not been ascertained.

When the hen begins to sit, a new laid egg, which has not been allowed to cool, will have the rudiments of the embryo formed some hours sooner than in the other eggs.

Having traced the formation of the egg itself through all the changes that take place from the time the yelk leaves the ovarium till it is impregnated, we are now to follow those changes that are met with during incubation till the embryo becomes a completely formed chicken.

In 4 *hours* after incubation the outer edge of the areola had become enlarged, and that part of it next the molecule appeared darker. One end of the molecule appeared like a white line, the first rudiments of the embryo. Plate XXXIII. fig. 1, 2, 3.

In 8 *hours*, the white line was found to be extended, and the rudiments of a brain and spinal marrow were formed, surrounded by a membrane, which afterwards becomes amnion.

The areola had extended itself, and the surface beyond the line which formed its boundary had acquired the consistence of a membrane, and had also a distinct line by which it was

circumscribed. This I shall call the outer areola. In the space between these two areolas, there were distinct dots of an oily matter. This extension of membrane to the outer areola lies under the inner membrane of the yelk, and can readily be removed entire. Plate XXXIII. fig. 4, 5, 6.

In 12 *hours*, the rudiments of the brain were more distinct, as well as of the spinal marrow. These parts were placed upon a black ground in vinegar, and hardened; the upper end showed the tuberculum annulare of the brain, from which passed down two semi-transparent lines resembling an appearance peculiar to the spinal marrow of the bird. Plate XXXIII. fig. 7, 8, 9.

In 16 *hours*, there was a farther advance in the structure of all these parts. Plate XXXIV. fig. 1, 2, 3.

In 24 *hours*, a still greater increase. Plate XXXIV. fig. 4, 5, 6.

In 36 *hours*, the head was turned to the left side. The cerebrum and cerebellum appeared to be distinct bodies; the iris was seen through the pupil of the eye. The intervertebral nerves were nearly completely formed; those nearest the head the most distinct. A portion of the heart was seen.

At this period, under the inner areola, apparently at the termination of the spinal marrow, a vesicle had begun to protrude. In some eggs it is seen earlier than in others, and has been observed before the heart had become visible. Plate XXXIV. fig. 7, 8, 9.

In 2 *days* 12 *hours*, the spinal marrow was found to have its posterior part enclosed; the auricles and ventricles of the heart were seen, the auricles filled with red blood. An arterial trunk from the left ventricle gave off two large

vessels, one to the right side of the abdomen of the embryo, the other to the left, sending branches over the whole of the areolar membrane, which was bounded on each side by a large trunk carrying red blood; but the two trunks did not unite, there being a small space on one side rendering the circle incomplete.

The vesicle was somewhat increased in size; it lay in the lower part of the abdomen, the parietes of which were not yet formed. Plate XXXV. fig. 1, 2, 3.

In 3 days, the outer areola had extended itself over $\frac{1}{3}$ of the circumference of the yelk, carrying the marginal arteries along with it to the outer edge, but diminished in size. The brain was much enlarged, consisting of four cavities containing a fluid, the cerebellum still the largest. The spinal marrow and its nerves were more perfectly formed. The eye appeared only to want the nigrum pigmentum.

The right ventricle of the heart contained red blood: the arteries could be traced to the head: the rudiments of the wings and legs were formed: the vesicle was farther enlarged, but its vessels did not carry red blood. It had forced its way out through the external covering of the yelk, and opened a communication through this slit, by which a part of the albumen was admitted to mix itself with the yelk, and give it a more oval form. At this period the embryo is generally found to have changed its position, and to be wholly turned on the left side. Plate XXXV. fig. 4, 5, 6.

In 4 days, the vesicle was more enlarged and more vascular, its vessels containing red blood.

The optic nerve and nigrum pigmentum of the eye were visible; the other parts had become more perfectly formed,

The outer areola had extended itself half over the yelk, which had now become still more encreased in size, a greater portion of albumen having become mixed with it. Plate XXXV. fig. 7, 8, 9.

In 5 days, the membranous bag that formed the vesicle had acquired a great size, and become exceedingly vascular in its coats; the cavity contained a fluid. The yelk had become thinner in its consistence, more of the albumen having been mixed with it. Plate XXXVI. fig. 1, 2, 3.

In 6 days, the vascular membrane of the areola had extended farther over the yelk. The vesicle at this time had suddenly expanded itself in form of a double night-cap over the yelk and its coverings, beginning to enclose the embryo.

This change is so rapid, that it has been with difficulty detected, and different accounts have been given of the mode in which it takes place.

The amnion contained water, in which the embryo floated, suspended by the vessels supplying the areolar and vesicular membrane.

The brain had become enlarged to the size of the body of the embryo: its vessels were distinctly seen. The two eyes equalled in size the whole brain: the marsupium was seen covered with nigrum pigmentum. The vessels of the cerebellum could be traced into the convolutions of the pia mater.

The parietes of the thorax and abdomen had begun to form; the wings and legs were nearly completely formed, as well as the bill. At this period muscular action was first noticed. Plate XXXVI. fig. 4, 5, 6.

In 7 days, the vesicle having extended over the embryo,

had begun to enclose the areolar covering of the yelk, and a pulsation was distinctly seen in the trunk that supplied the vesicular bag with blood. The pulsations were 79 in a minute while the embryo was kept in the temperature of 105°, but when the temperature was diminished the pulsation ceased, and when restored was reproduced. The pulsation was kept up, by attention being paid to the temperature, for 36 hours. Muscular action had become vigorous in the limbs.

When the embryo was completely immersed, although the temperature of the water was 108°, the pulsations immediately ceased, the blood being no longer aerated. Plate XXXVI. fig. 7, 8, 9.

In 8 *days*, the anastomosing branches of the vesicular circulation had the arterial pulsation very strong in them. Plate XXXVII. fig. 1, 2, 3.

In 9 *days*, the vesicle had nearly enclosed the yelk, but not intirely; for when the embryo was turned upon its back, and the opposite surface examined, a portion of yelk was unenclosed, and beyond it some of the albumen was met with not mixed with the yelk. Plate XXXVII. fig. 4, 5, 6.

In 10 *days*, the vesicle was opened, and the upper half turned aside. When the amnion, which had become full of water, was opened and the embryo taken out, the thorax was found completely enclosed; the roots of the feathers were distinct, and the passage for the areolar as well as the vesicular vessels exposed.

In 14 *days*, the yelk remained out of the body. When the thorax and abdomen were opened, and the heart as well as the lobes of the liver turned aside, the trunks of the blood-vessels could be traced to the heart; but as the arteries

immediately after death become empty and the veins remain full, the vein from the vesicle, and that from the areola were the most conspicuous.

In 18 *days*, the greater part of the yelk was drawn into the body.

In 20 *days*, the chick was completely formed; the yelk was entirely drawn in, and only some portions of the membrane of the vesicle appeared externally. The yelk passed into the intestine a little way above the openings of the cœca.

Having traced the progress of the formation of the chick step by step, from the first appearance of the molecule found on the yelk before it leaves the ovarium to the complete evolution of all its parts, and its leaving the shell, I shall now take advantage of this investigation, which, illustrated as it is by Mr. Bauer's microscopical drawings, will stand long without a parallel.

Although the processes by which the human fœtus and that of quadrupeds are formed, differ in many essential particulars from those employed in the bird, some circumstances are common to both; one of these, the mode in which the vesicle bursts the membranes of the yelk is distinctly seen in the embryo in the egg hatched out of the body, and explains what takes place where the embryo is attached to the uterus, which otherwise could not have been ascertained.

In a former paper, I have shown the formation of the ovum in the corpus luteum; I have traced this ovum into the uterus, where it became concealed in the soft flocculent bed of efflorescent coagulable lymph prepared to receive it. That it is afterwards enveloped in this moss-like covering, has been demonstrated by my revered master in Human Anatomy,

Dr. WILLIAM HUNTER; but in what manner the embryo breaks its amnion, and opens a communication with the uterus, is a question on which Dr. HUNTER is silent: indeed it is one that could not be touched upon till it was ascertained that the ovum, when it arrived at the uterus, was completely enclosed in the amnion.

Having been the first to ascertain that curious fact, and having arrived at that knowledge through the microscopical observations of Mr. BAUER, I am peculiarly gratified, that the same powers, exerted in another correspondent investigation, should have enabled me to give the solution.

In the ovum of the hen, the rudiments of the embryo require nourishment from one source, aeration from another. In the human ovum one source supplies both; but in either case there is a necessity for a communication between the blood vessels of the embryo and the source of their blood's aeration; and this is effected in exactly the same manner in the human species, the quadruped, and the bird. Out of this the embryo acquires a mode of suspension in the amnion which secures it from injury, both in the bird and quadruped. The mode is as follows: that bag, which is afterwards in the human species and in quadrupeds to become the urinary bladder, is enlarged to a certain size with such rapidity, that it bursts the amnion, which is prepared to be so ruptured, and the arteries lying upon its two sides are carried directly into contact with the chorion, and there the placenta is formed in the open space between the two edges of the chorion. This is exactly similar to the vesicle in the chick bursting the covering of the yelk, and forming the enveloping bag. It is deserving of remark, that in the human ovum, and that of

quadrupeds, there is a natural opening in the external membrane; in the bird, the natural opening is in the internal one, which in its structure bears a resemblance to the chorion.

EXPLANATION OF THE PLATES.

Plate XXXII.

Fig. 1. The ovarium of a hen that had been laying for some time. Natural size.

Fig. 2. A yelk taken out of its bag, and lying in its natural position horizontally. Natural size.

Fig. 3. A small portion of the yelk, with the molecule lying on the bare surface of the yelk within the aperture of the inner membrane of the yelk; the external membrane is turned aside, and shows a concave aperture which circumscribes exactly the molecule. Magnified 5 diameters.

Fig. 4. The bare molecule after being hardened in distilled vinegar, laid on a black ground. Magnified 10 diameters.

Fig. 5. The globules of which the inner areola is chiefly composed. Magnified 400 diameters.

Fig. 6. An egg found in the oviduct of the same hen from which the above ovarium was taken. Natural size.

Fig. 7. The same egg, its soft and semi-transparent shell being removed, the albumen and yelk are enclosed in the double membrane which lines the shell. Natural size.

Fig. 8. A small portion of the yelk of the same egg, with the molecule lying on its bare surface within the orifice of the inner membrane, and the external membrane turned aside. Magnified 5 diameters.

Fig. 9. The same molecule hardened, and laid on a black ground. Magnified 10 diameters.

Fig. 10. A new laid egg. Natural size.

Fig. 11. A small portion of the yelk of the same egg, with the molecule lying on its bare surface, and the external membrane of the yelk turned aside. Magnified 5 diameters.

Fig. 12. The molecule hardened in vinegar and laid on a black ground. Magnified 10 diameters.

Plate XXXIII.

Fig. 1. *An egg, opened* 4 *hours* after incubation. Natural size.

Fig. 2. A small portion of the yelk, with the molecule and areola under the external membrane of the yelk. Magnified 5 diameters.

Fig. 3. The bare molecule, hardened and laid upon a black ground. Magnified 10 diameters.

Fig. 4. *An egg, opened* 8 *hours* after incubation. Natural size.

Fig. 5. A small portion of the yelk, with the molecule and enlarged areola lying on its surface under the external membrane. Magnified 5 diameters.

Fig. 6. The same molecule with its enlarged areola, hardened, and laid on a black ground. Magnified 10 diameters.

Fig. 7. *An egg, opened* 12 *hours* after incubation. Natural size.

Fig. 8. A small portion of the yelk, with the embryo and areola lying on its surface under the external membrane. Magnified 5 diameters.

Fig. 9. The same embryo and areola hardened and laid on a black ground. Magnified 10 diameters.

Plate XXXIV.

Fig. 1. *An egg, opened* 16 *hours* after incubation. Natural size.

Fig. 2. A small portion of the yelk, with the embryo and areola lying on its bare surface, and the external membrane of the yelk entirely removed. Magnified 5 diameters.

Fig. 3. The same embryo, and a portion of the areola laid on a black ground. Magnified 10 diameters.

Fig. 4. *An egg, opened* 24 *hours* after incubation. Natural size.

Fig. 5. A small portion of the yelk, with the embryo and areola lying on its surface, and the external membrane of the yelk entirely removed. Magnified 5 diameters.

Fig. 6. The same embryo, and a portion of the areola laid on a black ground. Magnified 10 diameters.

Fig. 7. *An egg, opened* 36 *hours* after incubation. Natural size.

Fig. 8. A small portion of the yelk, with the embryo and areola lying on its surface, and the external membrane of the yelk removed. Magnified 5 diameters.

Fig. 9. The same embryo, and a portion of the areola laid on a black ground. Magnified 10 diameters.

Plate XXXV.

Fig. 1. *An egg, opened two days and twelve hours* after incubation. Natural size.

Fig. 2. A portion of the yelk, with the embryo lying in its natural position in the centre of the vascular areola, and the external membrane of the yelk removed. Magnified 6 diameters.

Fig. 3. The same embryo, with the principal branches of its blood-vessels, turned upon its back, and laid upon a black ground. Magnified 6 diameters.

Fig. 4. *An egg, opened 3 days* after incubation. Natural size.

Fig. 5. A portion of the yelk and vascular areola, with the embryo in its centre, lying entirely on its left side, which is its natural position. Magnified 6 diameters.

Fig. 6. The same embryo turned on its right side, and laid on a black ground. Magnified 6 diameters.

Fig. 7. *An egg, opened 4 days* after incubation. Natural size.

Fig. 8. The embryo in its amnion, and with its vesicle removed from the yelk, and laid in its proper position on a black ground. Magnified 4 diameters.

Fig. 9. The same embryo without its amnion, turned on its back, and laid on a black ground. Magnified 4 diameters.

PLATE XXXVI.

Fig. 1. *An egg, opened 5 days* after incubation. Natural size.

Fig. 2. The embryo in its amnion, and with its vesicle removed from the yelk, and represented in its natural position. Magnified 3 diameters.

Fig. 3. The same embryo taken out of its amnion, and turned upon its back, having on its right side the increasing vesicle, and on its left a small portion of the areolar membrane with the principal trunk of the blood-vessels. Magnified 3 diameters

Fig. 4. *An egg, opened six days* after incubation. Natural size.

Fig. 5. The embryo in its amnion, and with its vesicle removed from the yelk, and represented in its natural position. The vesicle at this period forms an adhesion with the amnion at the lower extremity. Magnified 3 diameters.

Fig. 6. The same embryo turned on its back, having on its right side a small portion of the vesicular, and on its left, of the areolar membrane, with the principal trunks of their blood-vessels. Magnified 3 diameters.

Fig. 7. *An egg, opened the seventh day* after incubation. Natural size.

Fig. 8. The embryo in its amnion, and with its vesicle removed from the yelk, and represented in its natural position. The vesicle, which at this period covers entirely the embryo, is here turned downwards, and shows the increasing adhesion with the amnion. Magnified 2 diameters.

Fig. 9. The same embryo extracted from its amnion, and turned upon its back, having on its right side a small portion of the vesicular, and on its left, of the areolar membrane, with the principal trunks of their blood-vessels. Magnified 2 diameters.

PLATE XXXVII.

Fig. 1. *An egg, opened* 8 *days* after incubation. Natural size.

Fig. 2 The embryo within its amnion, in its natural position, and with a portion of the vesicular and areolar membranes, with the principal trunks of their blood-vessels. Magnified 2 diameters.

Fig. 3. The same embryo turned on its back, having small portions of the vesicular and areolar membranes protruding from the enclosed part of the abdomen. Magnified 2 diameters.

Fig. 4. *An egg, opened nine days* after incubation. Natural size.

Fig. 5. The same egg turned more to its right side, to show that at this period, neither the vesicular, nor the areolar membranes, yet enclose the whole yelk, nor is the albumen entirely absorbed. Natural size.

Fig. 6. The embryo of the same egg, with a portion of the vesicular and areolar membranes, and their principal blood-vessels; the amnion is opened as far as the vesicular adhesion admits. Magnified 2 diameters.

Fig. 7. *An egg, opened ten days* after incubation. Natural size.

Fig. 8. The same egg with the external half of the vesicle removed, showing the embryo distinctly in its amnion, and the inner half of the vesicle, with its blood-vessels covering the whole. Natural size.

Fig. 9. The embryo of the same egg, with the amnion and the vesicle entirely removed, to show the opening in the abdomen, from which portions of the vesicular and areolar membranes and turns of the intestines are protruding; the roots of the feathers are now visible. Magnified 2 diameters.

Plate XXXVIII.

Fig. 1. *An egg, opened fourteen days* after incubation. Natural size.

Fig. 2. The same egg, with the external half of the vesicle removed, to show the embryo in its amnion, and that the yelk is not yet entirely enclosed by the areolar membrane. Natural size.

Fig. 3. The embryo of the same egg; its thorax and ab-

domen opened; the heart and the lobes of the liver turned aside to show the course of the principal trunks of the blood-vessels which go to the vesicle and to the areolar membrane; the yelk, at this period, is still entirely without the body of the embryo. Magnified 2 diameters.

A.A. a portion of the amnion. B.B.B. the vesicle. C.C. a portion of the areolar membrane.

Plate XXXIX.

Fig. 1. *An egg, opened eighteen days* after incubation. Natural size.

Fig. 2. The same egg, with the external half of the vesicle removed. Natural size.

Fig. 3. The chick taken from the same egg; the thorax and abdomen laid open to show that the greater part of the yelk is now drawn into the body. Natural size.

Plate XL.

Fig. 1. *An egg, opened twenty days* after incubation, the vesicle and amnion entirely removed, to show the natural position of the perfectly formed chick. Natural size.

Fig. 2. The chick taken out of the same egg, to show that the yelk is now entirely drawn into its body, only some small portion of the vesicular membrane protruding from the orifice of the abdomen, which is not yet quite closed. Natural size.

Fig. 3. The same chick opened, to show the natural position of the viscera and the yelk at this period of the incubation. Natural size.

PLATE XLI.

Fig. 1. *A chick, after being hatched 24 hours;* the thorax and abdomen opened, to show the natural position of the viscera and yelk; the crop at that time is full of corn, and other food. Natural size.

Fig. 2. The viscera of the same chick; the heart turned upwards, and the two lobes of the liver laid aside, to show that the vena cava is a continuation of the vesicular vein, and terminates in the right auricle of the heart, after receiving the venæ hepaticæ. Magnified 2 diameters.

The areolar vein terminates in the vena portarum, which is not seen in this view.

Fig. 3. A posterior view of the gizzard and œsophagus, to show the origin of the duodenum, and the course of the intestinal canal, of the same chick. Magnified 2 diameters.

Fig. 4. An anterior view of the rectum, and colon, to show at A, where the great mass of the yelk has been cut off. Magnified 2 diameters.

XXVI. *Some observations on Corrosive Sublimate.* By John Davy, *M. D. F. R. S.*

Read June 6, 1822.

I am not aware that the operation of light on corrosive sublimate, has yet been minutely considered. It is known that the *liquor hydrargyri oxymuriatis* of the London Pharmacopœia is decomposed by light; it has been stated, that the compound itself, when exposed to light, undergoes the same change; and it has been recommended, in consequence, to keep it in black bottles.

With a view to acquire some precise information on this subject, the following experiments were instituted.

A few grains of corrosive sublimate, in the state of fine powder, were exposed to sunshine for 14 days in a small glass tube, corked and sealed. No change was then produced, as was proved by the corrosive sublimate dissolving entirely in muriatic acid.

A solution of corrosive sublimate in distilled water was exposed to sunshine for the same length of time. A thin white crust formed, which was found to be calomel; and traces of free muriatic acid were detected in the solution.

Some *liquor hydrargyri oxymuriatis* and solutions of corrosive sublimate in rectified spirit and in ether were exposed to sunshine for the same time. In the former, a considerable crust

of calomel formed; whilst in the two latter no change took place.

Some oil of turpentine was poured on corrosive sublimate, and exposed to sunshine for a week. The fluidity of the oil was slightly impaired, but the corrosive sublimate was unaltered.

To a saturated aqueous solution of corrosive sublimate, a few drops of muriatic acid were added, and to another saturated solution, a small quantity of muriate of ammonia. No change was produced in these solutions by the action of light during exposure for three weeks.

From these experiments it may be deduced, that light alone has not the power of decomposing corrosive sublimate, and that it does not produce the effect, excepting when aided by affinities of a complicated nature.

In confirmation of this conclusion, I beg leave to relate some other experiments made with a view to illustrate it, and which, I trust, will not prove uninteresting in themselves.

The solubility of corrosive sublimate in water and alcohol is differently stated by authors. In one experiment, made with as much accuracy as was in my power, 37 grains of distilled water were required to dissolve 2 grains of corrosive sublimate at the temperature 57° FAHRENHEIT, which is in the proportion of about 5.4 per cent. Its degree of solubility increases greatly with the temperature, but in what ratio, it is not easy to ascertain with precision, owing to the trivial circumstances by which the crystallization of the salt is affected.

Alcohol, of specific gravity .816, at 60°, dissolves, I find, half its weight of corrosive sublimate: thus 10 grains of it

were completely dissolved in 20 grains of alcohol; one grain more was added and gentle heat applied; it dissolved, but on cooling separated in the form of minute spicular crystals. The saturated solution was of specific gravity 1.08.

Ether, I find, dissolves nearly one-third its weight of corrosive sublimate: thus, 20 grains of sulphuric ether of specific gravity .745, took up 7 grains. It was of the same specific gravity as the alcoholic solution, viz. 1.08. It may be worthy of notice, that the solvent power of ether, as well as I could judge from my experiments, is not increased by elevation of temperature, or diminished by its reduction. The boiling point, too, of the solution and of pure ether, appears to be the same. In the act of ebullition, the solution seems to be decomposed: where a bubble of ether is formed, there a minute portion of corrosive sublimate is precipitated, owing, probably, to the property just mentioned.

Though no change is produced in corrosive sublimate by oil of turpentine under the influence of light, a considerable action and mutual decomposition is the result of a mixture of the two being gently heated. On the application of a gentle heat to corrosive sublimate in fine powder moistened with oil of turpentine, the mixture becomes of a fawn colour; there is a sudden and considerable elevation of temperature, acid fumes are generated, calomel is formed, and if the heat be raised, a residue of carbon is obtained. The results appear to be modified by the proportions of the two substances: when the quantity of corrosive sublimate is large, the whole of the oil appears to be completely decomposed, and the products are, liquid muriatic acid, calomel, and charcoal: when the oil is in excess, the part that escapes decomposition, passes over

impregnated with muriatic acid; and, judging from its smell, appears to contain a minute quantity of artificial camphor. From the calomel that is mixed with it, and which rises with it, forming a very dense white vapour, it is not easy, on a small scale of experimenting, to demonstrate the actual formation of camphor.

I believe, that changes very similar take place, when corrosive sublimate is heated with other oils, both volatile and fixed. Of the latter, I have tried one only, oil of almonds; when heated with corrosive sublimate, it immediately blackens, and acid fumes are disengaged. Of the former, I have tried, in addition to the oil of turpentine, the oils of cinnamon, nutmeg, juniper, carraway, peppermint, and cloves. On the application of heat to the first four, mixed with corrosive sublimate, the colour changes to brown, and then to black, and copious acid fumes are disengaged. The mixture of oil of cloves and corrosive sublimate, when gently heated, becomes of a beautiful bright purple, and the heat being raised, acid fumes are disengaged, an oil of the same purple colour distils over, and the residue becomes black. The purple oil appears to be a compound of muriatic acid gas and oil of cloves; and it can be produced either by passing muriatic acid gas into oil of cloves, or by agitating the oil with liquid muriatic acid. On the addition of corrosive sublimate to oil of peppermint, the colour of the oil is immediately changed to bright yellow; on the application of heat, it instantly blackens; a light purple oil volatilises with acid fumes, and calomel with carbonaceous matter remains. The production of a purple oil in this instance, appears to depend on the same cause as in the preceding, and it can be

formed in the same way by the action of muriatic acid or its gas.

In a paper published in the Philosophical Transactions for 1812, I have noticed the affinity of muriatic acid for corrosive sublimate. Muriatic acid of specific gravity 1.158, at 74°, dissolves, I find, twice its weight of corrosive sublimate. This solution may be considered as composed of 11 proportions of water, 1 muriatic acid,* and 1 salt. In the act of forming, heat is evolved. At 74° this solution is of specific gravity 2.412. When its temperature is lowered a few degrees, it suddenly becomes solid, and forms a mass of delicate needle crystals, which rapidly melt, when the containing vessel is held in the warm hand.

It is commonly stated in systematic works, that corrosive sublimate is soluble in the sulphuric and nitric acids, as well as in the muriatic acid. From the experiments which I have lately made, this does not appear to be the case. One-tenth of a grain of corrosive sublimate was added to 50 grains of nitric acid of specific gravity 1.45; kept some time at the temperature 90°, it did not diminish in bulk, nor did it appear to dissolve even at the boiling point of the acid, nor did the acid appear turbid on cooling, nor were any crystals deposited. A similar experiment was made with the same quantity of sublimate and 63 grains of concentrated sulphuric acid: at 90° the sublimate did not dissolve; and on the application of heat, fumes appeared, the salt rose through the acid, and a delicate crust of it was formed in the cool part of the vessel.

The experiments which I have made, and which I shall immediately relate, tend to corroborate an opinion long ago

* New System of Chem. Phil. by JOHN DALTON, vol. ii. p. 295.

entertained, that muriate of ammonia and corrosive sublimate are capable of uniting and of forming a double salt,* and to prove that similar compounds may be formed with some other muriates.

In the dry way, there appears to be an affinity exercised between corrosive sublimate and muriate of ammonia. A mixture of the two, in the proportions of 34. of the former, and 6.75 by weight of the latter, heated, forms a compound more fusible and less volatile than either ingredient separate; it may be kept liquid without volatilising by the gentle heat of a spirit lamp; on cooling, it exhibits a very light grey translucent mass of a faint pearly lustre; strongly heated, it sublimes, and appears to be partially decomposed, as traces of calomel and free muriatic acid are found mixed with the sublimate. This compound, formed of one proportion of each ingredient, has more the character of a chemical compound, than any other mixture of the two ingredients that I have tried.

In the moist way, the affinities of corrosive sublimate and muriate of ammonia are better marked, and some of the combinations of the two have tolerably well defined qualities. The following have the best claim to be considered distinct combinations of any which I have yet made:

No.	Water.	Muriate of Ammonia.	Corrosive Sublimate.
1	9.00 grs.	6.75	34.00
2	9.00	3.37	17.00
3	9.00	3.37	8.50
4	9.00	10.12	25.50

* The sal-alembroth of the alchemists was a compound of this kind.

No. 1, is liquid at 140°; on cooling, it forms a solid mass of needle crystals. No. 2, is liquid at 85°, and solid at 55°. In the liquid form at the temperature just mentioned, it is of specific gravity 1.98. No. 3, is liquid at 55°, and of specific gravity 1.58. No. 4, is liquid at about 105°; on cooling slowly to 60°, it deposits some crystals which are four-sided prisms, composed of facets alternately broad and narrow.

That corrosive sublimate and muriate of ammonia have a strong affinity for each other, is evident from the circumstance, that united, the solubility of the compound exceeds that of the most soluble ingredient. This is proved by the solubility of the 3d compound just described: farther in proof, it may be mentioned, that a saturated solution of muriate of ammonia at 60°, is capable of dissolving its own weight very nearly of corrosive sublimate, and that after this addition it is capable of taking up more muriate of ammonia; thus, 25.3 grains of such a saturated solution, after having taken up 25.1 grains of corrosive sublimate, dissolved 7 grains more of muriate of ammonia. From this experiment it would appear, that corrosive sublimate is about 17 times more soluble in a saturated solution of muriate of ammonia than in water, and not 30 times, as is stated by some authors.

The results of these experiments led me to make trial of some other muriates, as of baryta, magnesia, potash, and soda.

A saturated solution of muriate of baryta, formed of 20 grains of water and of 8.7 grains of the crystallized salt,* dissolved 16 grains of corrosive sublimate at 60°, and 4 grains

* 8.7 grains of this salt, heated nearly to redness, lost 1.4 grain water of crystallization.

more when gently heated; on cooling, a very few granular crystals were deposited. The solution was of specific gravity 1.9. After rest for several hours, it deposited a small number of minute and apparently cubical crystals.

31 grains of muriatic acid, of specific gravity 1.58, carefully neutralized with magnesia, dissolved 40 grains of corrosive sublimate, and, when gently heated, 25 grains more. This solution remained transparent on cooling. When 5 grains more of the sublimate were added, these too were dissolved when heated; on cooling, a good many spear-shaped crystals formed. The solution first made, when poured from one vessel into another, had an oily appearance, and was rather less fluid than concentrated sulphuric acid; it was of specific gravity 2.83; gently evaporated, greyish pellicles formed, which rapidly deliquesced on exposure to the air.

A saturated solution of muriate of potash formed of 21 grains of water and 7 grains of dry muriate of potash, gently heated, dissolved 8 grains of corrosive sublimate. On cooling to 60°, it deposited only a very few needle crystals; but when cooled by means of ether to 50°, it became nearly solid, a mass of delicate needle crystals admitting of being inverted.

A saturated solution of common salt, composed of 20 grains of water and 7 of salt, dissolved 32 grains of corrosive sublimate at 60°; gently heated, 3 grains more were dissolved, and remained in solution on cooling; but, on a farther addition of corrosive sublimate, the solution formed by heat, deposited, on cooling, small rhomboidal crystals. The solution, containing 35 grains of sublimate, was of specific gravity 2.14. Like muriate of ammonia, the solubility of common

salt in water appears to be increased by combining with corrosive sublimate, but in a less degree.

It appeared probable, that a compound of common salt and corrosive sublimate might be formed in the dry way. 7.5 grains of the former and 34 of the latter were heated together: no proof was afforded of combination having taken place; the corrosive sublimate, on the application of heat, rose as readily as if heated by itself.

I thought it possible that water containing common salt, might have the power of dissolving chlorides that are insoluble in water alone; but experiment did not confirm the conjecture in the instance of calomel and horn silver.

May not the compounds of corrosive sublimate and common salt, muriate of magnesia and baryta, respectively, be considered as constituted of one proportion of each ingredient? The definite nature of the compounds with muriate of ammonia and potash, are perhaps more questionable.

It is worthy of remark, that all these compounds exhibit the properties of the most active constituent, or of that, the saturating power of which is greatest; so that, though the quantity of corrosive sublimate dissolved in any one instance is large, it modifies very little the character of the solution.

To conclude; it would appear from the preceding experiments that these menstrua, which have a strong affinity for corrosive sublimate, prevent its decomposition when exposed to light, as the muriates, alcohol, and ether; and, on the contrary, that those solvents which exercise a weak affinity on it, and have a stronger affinity for muriatic acid, as water, and exceedingly dilute alcohol, aid the decomposing power of light.

The practical application to be deduced, relative to the formula for the *liquor hydrargyri oxymuriatis*, is obvious, and does not require to be pointed out.

Fort Pitt, Chatham,
May 14, 1822.

XXVII. *On the state of water and aëriform matter in cavities found in certain crystals.* *By Sir* Humphry Davy, *Bart. P. R. S.*

Read June 13, 1822.

There are few enquiries in natural science more calculated to awaken our curiosity, than those relating to the changes which the matter composing the surface of our globe has undergone. The imagination is excited by the magnitude of the operations, by the obscurity of the phenomena, and the remoteness of the time at which they occurred; and all the intellectual powers are required to be brought into activity to find facts or analogies, or to institute experiments, by which they may be referred to known causes.

The crystallizations constituting the whole of the rocks which are usually called primary, and those found in such abundance, even in the rocks which are termed secondary, prove that a considerable part of the materials of the surface of the globe must have been either fluid or aëriform; for these are the only states from which the regular arrangements of the molecules of bodies constituting crystals, can be produced.

Geologists are generally agreed, that the greater number of the crystalline mineral substances must have been previously in a liquid state; but different schools have supposed different causes for their solution; some attributing this effect principally to the agency of water, others to that of heat.

When, however, it is considered, that the solvent power of water depends upon its temperature, and its deposition of solid matters upon its change of state or of temperature; and that being a gravitating substance, the same quantity must always belong to the globe, it becomes difficult to allow much weight to the arguments of the Wernerians or Neptunists, who have generally neglected, in their speculations, the laws of chemical attraction.

There are many circumstances, on the contrary, favourable to that part of the views of the Huttonians or Plutonists, relating to the cause of crystallization; such as the form of the earth, that of an oblate spheroid flattened at the poles; the facility with which heat, being a radiating substance, may be lost and dissipated in free space; and the observations which seem to show the present existence of a high temperature in the interior of the globe.

I have often, in the course of my chemical researches, looked for facts or experiments, which might throw some light on this interesting subject, but without success, till about three years ago; when, in considering the state of the fluid and aëriform matters included in certain crystals, it appeared to me, that these curious phenomena might be examined in a manner to afford some important arguments as to the causes of the formation of the crystal.

It is well known that water, and all fluids at usual temperatures, are more expansible by heat than siliceous or other earthy matters; and supposing these crystals to have been formed, and the water or fluid inclosed in them, at a pressure and temperature not very unlike those of our existing atmosphere, this fluid ought to fill nearly the same space as when

included, and the elastic fluid confined with it, supposing it non-absorbable, ought to be in the same state of density. On the contrary, if the earthy matter and the fluid separated from each other under a much higher temperature than that now belonging to the surface, a certain vacuum might be expected in the cavity from the contraction of the fluid, and if any gas were present, a considerable rarefaction of it; and though, supposing a much higher temperature on the surface of the globe, the atmosphere formed by aqueous vapour must have had much greater absolute weight, which, as liquids are *compressible*, must have influenced the volume of the fluid at the time it was inclosed, a circumstance which would render it impossible to draw any conclusion as to the exact temperature, yet still the experiments appeared to offer, on any view, interesting results; and I was the more desirous of performing them, as I believe the nature of the fluid and aëriform matters included in rock crystals and other siliceous stones, has never been accurately ascertained.

Having purchased some crystals, and having had others committed to my care by the liberality of my Brother Trustees of the British Museum, and of my friend Professor BUCKLAND, I proceeded to make the necessary experiments upon them. It will be improper for me to take up the time of the Society by a minute description of my manipulations. Holes were drilled in the crystals by the use of diamonds, generally by Mr. NEWMAN, under distilled water, or mercury, the gas was expelled by the introduction of wires, and the fluids included in the cavities were drawn out by means of fine capillary tubes, and experiments were afterwards made to determine the space they occupied, which had been accurately measured and

marked upon the crystal. The chemical nature of the fluid and gas was determined by processes which were necessarily difficult from the smallness of the quantities operated upon; but which are too well known to the chemical philosophers of this Society to need description.

The first three crystals that I examined were from Schemnitz, in Hungary; the cavities that they contained were proved not to be permeable to the atmosphere, by exposure to rarefied air, alone, and under water, in the receiver of an air pump, a circumstance which it was necessary always to attend to, in order to render the experiment availing.

A cavity in one of the crystals was pierced under oil, three under distilled water, and one under mercury. In all of them the fluid rushed in when the cavity was opened, and the globule of elastic fluid contracted so as to appear from six to ten times less than before the experiment. The fluid in all the crystals (in two it was minutely examined) was found to be water nearly pure, containing only a minute portion of the alkaline sulphates. The elastic fluid, as well as I could ascertain from the very minute quantities I could procure, appeared to be azote, unmixed with any other substance.

The largest cavity, which was in the crystal put into my hands by Professor BUCKLAND, contained a space equal to 74.5 grains of mercury; the water in it equalled in volume 48.1 grain measures of mercury; and the globule of air, after the experiment, equalled in diameter a globule of mercury weighing 4.2 grains, so that the elastic fluid had contracted at least between six and seven times.

In the other experiments, the cavities being much smaller, the quantities of air and fluid could not be accurately mea-

sured; but there seemed to be nearly the same relation between the space filled by fluid, and that containing aëriform matter; and in all of them the contraction of the globule of aëriform matter was evidently greater, and in one instance to less than $\frac{1}{10}$ of its original bulk.

The fourth crystal that I experimented upon was of unknown locality; but I have reason to believe that it was from Guanaxuato, in Mexico, as it strongly resembled some that Mr. Heuland showed me from that place. The cavity in it was extremely small, and when pierced into, under distilled water, the globule of gas, from being $\frac{1}{8}$ of an inch in diameter,* diminished so as to be less than $\frac{1}{25}$; so that its rarefaction was much greater in this than in the other instances; the water was too small in quantity to be minutely examined; it seemed to be nearly pure, producing a cloudiness barely perceptible in solutions of nitrate of silver and muriate of baryta.

It was an interesting point to ascertain whether the same circumstances occurred in productions found in rocks which have been generally considered as of igneous origin, such as the basaltic rocks in the neighbourhood of Vicenza, the chalcedonies of which so often afford included water. I found it much more easy to make experiments of this kind, and to procure specimens, which were abundantly supplied to me from the same sources as those I have just referred to; and though some of these specimens proved to be permeable to the atmosphere, and to have been filled with water artificially,

* I have not thought it necessary to refer to the heights of the barometer and thermometer in these experiments, as it is impossible to gain any other than general results, upon quantities in which differences arising from atmospheric temperature and pressure, would be quite unappreciable.

yet many occurred, in which the sides of the cavity were absolutely impervious to air or water.

The results that I obtained were very analogous. Water containing very minute quantities of saline impregnations, occasioning barely a visible cloudiness in solutions of silver and of muriate of baryta, was found to be the fluid; the gas was azote, but it was in a much more rarefied state than in the rock crystals, being between 60 and 70 times as rare as atmospheric air.

The quantity of water was to the void space in greater proportion than in the rock crystals. In the instance in which the most accurate experiment was made, namely, on the great specimen preserved in the collection of the British Museum, and which weighed 380 grains, the quantity of water was 29.9 grains, the space occupied by aëriform matter was equal to 11.7 grains of water, the volume of the globule of gas at the common pressure was to that of its rarefied volume as 1 to 63.

It occurred to me that atmospheric air might have been originally the elastic fluid included in these siliceous stones and in the crystals, and that the oxygene might have been separated from the azote by the attraction of the water, and a direct experiment seemed to confirm this idea. A chalcedony which had been bored was placed in water free from air under a receiver, which was exhausted till a portion of gas from the interior of the crystal had escaped into a proper receptacle. This gas examined by nitrous gas, was found to contain nearly as much oxygene as atmospheric air; so that there is every reason to believe that the water had emitted oxygene during the exhaustion.

I endeavoured to find some calcareous secondary rocks, or crystals belonging to them, containing cavities, on which experiments of the same kind might be made; but in a number of trials I have as yet found none impermeable to the atmosphere; and the cavities of such, when bored, are always found to contain atmospheric air in a common state of density.

I was surprised to find that this was the case even with cavities in calcareous spar in the centre of a lime-stone rock; yet these cavities which contained atmospheric air did not fill with water when the stone was placed in water under an exhausted receiver. When however it was dry, and placed in a receiver alternately exhausted and filled with hydrogene, the air that was produced by piercing the cavities, was found mixed with hydrogene; proving that the substance of the stone was permeable to elastic fluid.

I hope soon to be able to make further researches on this subject; but in reasoning upon the vacuum, or rarefied state of the aëriform matter in the cavities of these rock crystals and chalcedonies, it appears difficult to account for the phænomenon, except on the supposition of their being formed at a higher temperature than that now belonging to the surface of the globe; and the most probable supposition seems to be, that the water and the silica were in chemical union, and separated from each other by cooling.

Water in the temperature of the arctic winter is constantly a crystallized body. As a fluid, its solvent powers are increased as its heat becomes higher, and when elastic, the density of its vapour is exalted in proportion to its heat; so that an atmosphere of steam, supplied from an indefinite

source above water, would render it capable of receiving a very high degree of heat. Lime retains water in combination at a heat above 250° FAHRENHEIT; baryta retains it (even under ordinary pressures) at a strong red heat, and fuses with it. It is extremely likely that a liquid hydrate of silica would exist, under pressure, at high temperatures; and like all liquid bodies in the atmosphere, would probably contain small quantities of atmospheric air; and such a supposition only is necessary to account for the phænomena presented by the water in rock crystal and chalcedony.

As, however, steam or aqueous vapour may be considered as having a share in these results, if it be supposed included in the cavity, no exact conclusions can be drawn from the apparent degree of contraction of the water; particularly as the late ingenious researches of Mr. PERKINS show, that water is much more compressible than was formerly imagined; and the volume of water, however high its temperature, must be influenced by the pressure to which it is exposed; so that a certain compressing weight may not only impede, but altogether counteract the expansive force of heat.

Many speculations might be indulged in on this subject, but I shall not at present enter upon them; and I shall conclude by observing, that a fact, which has been considered by the Neptunists, above all others as hostile to the idea of the igneous origin of crystalline rocks, namely, the existence of water in them, seems to afford a decisive argument in favour of the opinion it has been brought forward to oppose.

June 1, 1822.

APPENDIX.

SINCE the foregoing pages were communicated to the Royal Society, I have made some new experiments on the same subject; all of them, except two, offered results of the same kind as those I have detailed, and upon such I shall not enter: but these two, from their peculiarity, will not, I trust, be thought unworthy of a particular notice.

In examining, with Mr. HEULAND, the beautiful specimens of rock crystals in the collection of CHARLES HAMPDEN TURNER, Esq. I observed one crystal which, Mr. HEULAND informed me, was from La Gardette, in Dauphiné, that contained a considerable cavity, in which there was a viscid brownish liquid, resembling in its appearance and consistence linseed oil. As the void space or cavity filled with aëriform matter appeared considerable in proportion to the fluid, I expressed a desire to pierce the crystal; and Mr. TURNER, hearing of my wish, was so kind as to gratify it in the most polite and liberal manner, by presenting to me the specimen. With Mr. NEWMAN's assistance I made the usual experiments upon it. The cavity was pyramidal, and nearly the third of an inch in diameter. I soon ascertained that the fluid was not water, as it congealed and became opaque at a temperature of 56°. When the crystal was pierced under distilled water, the water rushed in and entirely filled the cavity, so that no other aëriform matter but the vapour of the substance could have been present; the water was rendered white and cloudy, apparently by the substance. I endeavoured to collect some of it for chemical examination, but it was too small in quantity (not equalling in volume $\frac{1}{6}$ of the volume of the cavity,) to be submitted to analysis. It swam on the water, had

no distinct taste, but a smell resembling naphtha; a portion of it taken out mixed with the water, when exposed to heat acted like fixed oil, and it seemed to have a high temperature of ebullition. It inflamed, producing a white smoke.

The fact, of almost a perfect vacuum existing in a cavity containing an expansible but difficultly volatile substance, may be considered as highly favourable to the theory of the igneous origin of crystals: the other experiment is of a nature entirely different, though its result *may* be explained in the same supposition.

In examining a crystal in the collection of the Royal Institution, and which from its characters I believe to be from Capaó d'Olanda, Province of Minas Geraes, Brazil, I observed that the quantity of aëriform matter was unusually small in proportion to the quantity of fluid, in two or three cavities not occupying $\frac{1}{10}$ or $\frac{1}{12}$ of the space; and from the peculiarity of its motion, it appeared to be more likely to be compressed, than rarefied elastic fluid; and in piercing the sides of the cavities I found that this was the case; it enlarged in volume from ten to twelve times; the fluid was water, but the gas was too minute in quantity to be examined.

It will be interesting to ascertain under what circumstances, and in what situations crystals of this kind are found. If they be supposed of igneous origin, they must have been formed under an immense weight of atmosphere or fluid, sufficient to produce a compression much more than adequate to compensate for the expansive effects of heat, a supposition which, in consequence of Mr. PERKINS's experiments, already alluded to, may be easily formed.

July 6, 1822.

XXVIII. *Some experiments on the changes which take place in the fixed principles of the egg during incubation.* By William Prout, *M. D. F. R. S.*

Read June 20, 1822.

In the year 1816, I was induced to commence a series of experiments on the egg during incubation, with the view of ascertaining the nature of the changes which take place during that process. My inquiry was chiefly limited to the fixed principles, namely, the earthy and saline matters; but my attention was more particularly directed to the source whence the earthy matter, constituting the skeleton of the chick, was derived.

With these views, the egg was analyzed in its recent and unaltered state, and at the end of the first, second, and third weeks of incubation. My experiments were chiefly confined to the eggs of the domestic fowl, but have been likewise partially extended to those of the duck and turkey. The investigation has been renewed, and the experiments repeated at various intervals since the period above mentioned; but the difficulty of the subject, and various accidents, have prevented me from completing them till the present time; and the results, which, after all, are much less perfect than I could wish, I have now the honour of submitting to the Society.

Preliminary experiments on the egg in its recent and unaltered state.

The specific gravity of new laid eggs has been found to vary from 1.080 to 1.090. When kept for some time, eggs, as is well known, rapidly lose weight, and become so specifically light as to swim in water. This diminution of specific gravity, however, is only apparent, and depends on the substitution of air for a portion of the water of the egg which escapes; for it is obvious, that the specific gravity of the constituent principles of the egg must be rather increased than diminished by the escape of water. The following table shows the gradual loss of weight of an egg during a period of two years.

The original weight on the 19th May, 1820, the day it was laid, was 907.5 grains.

	Grains.	Loss per day.		Grains.	Loss per day.
19 May, 1820.	907.5		5 May, 1821.	648.7	.59
20 . . .	906.5	1.00	6	647.8	.90
24 . . .	901.3	1.30	5 December	488.2	.75
31 . . .	894.2	1.01	7 . . .	486.6	.80
8 June . .	886.6	.95	21 March, 1822.	413.5	.70
17 . . .	879.3	.81	25 April .	384.6	.82
27 . . .	870.7	.86	26 . . .	383.7	.90
19 July . .	848.5	1.01	17 May . .	365.2	.84
7 August .	829.6	.99	18 . . .	364.3	.90
26 . . .	810.8	.99	19 . . .	363.2	1.10
30 September	778.5	.92	Total Loss	544.3	.744 Mean

Hence we learn, that a moderately sized egg loses on an average about .75 grain in twenty-four hours, and that

uniformly during a very long period.* The loss appears to be somewhat greater in summer than in winter, owing, doubtless, to the difference in temperature, which, in the present instance, varied from 40° to 70°. On being broken, the whole of the contents of this egg were found collected at the smaller extremity in a solid state, but on being put into water, they absorbed a large portion of that fluid, and assumed an appearance not much unlike those of a recent egg; the smell also was perfectly fresh.

The relative weights of the shell, albumen, and yelk of different eggs are very different. With the view of investigating this point, and of obtaining something like an average, the following experiments were made. The eggs were boiled hard in distilled water, and the different parts weighed immediately in their *moist* state.

	Shell and Membrane.	Albumen.	Yelk.	Total.
	Grains.	Grains.	Grains.	Grains.
	80	394.3	289	763.3
	108	593	273.5	974.5
	107.3	575.8	236.2	919.3
	71.5	516.5	215	803
	103	503.7	269.3	876
	107	515.3	273.4	895.7
	93.2	605.5	252.4	951.1
	92.7	515.7	257	865.4
	96.8	510.6	210.8	818.2
	77.6	567.4	241.5	886.5
Mean	93.7	529.8	251.8	875.3

If we suppose each of these eggs to weigh one thousand

* If the average of the above means be taken, the loss per day will be about .9 grain.

grains, the weights of the constituent principles of each, when reduced to this common standard, will be as follow:

	Shell and Membrane.	Albumen.	Yelk.
	Grains.	Grains.	Grains.
	104.8	516.6	378.6
	110.8	608.5	280.7
	116.7	626.3	257.0
	89.0	643.2	267.8
	117.6	575.0	307.4
	119.5	575.3	305.2
	98.0	636.6	265.4
	107.1	596.0	296.9
	118.3	624.0	257.7
	87.5	640.0	272.5
Average	106.9	604.2	288.9

Hence, if we suppose a recent egg to weigh one thousand parts, the relative proportions of the shell, albumen, and yelk, will be as 106.9, 604.2, and 288.9; and for the sake of easier comparison in all the subsequent experiments, the numbers are reduced to the above standard, or to the supposition, that the original weights of the eggs employed were, when just laid, exactly 1000 grains.

When an egg is boiled in water, it loses weight, particularly if it be removed from the water when boiling, and be permitted to cool in the open air;* the water also on examination will be found to contain a portion of the saline contents of the egg. The loss of weight from boiling is by no means constant, but has been found to vary from 20 to 30 grains, on

* When permitted to cool in water it sometimes gains a little in weight, owing to the absorption of water. Eggs placed in a strong solution of common salt are said to become highly saline throughout. This mode has been recommended for preserving them, but I have never tried the experiment.

the supposition, that the original weights of the eggs employed were 1000 grains. On the same supposition, also, it has been found, that the quantity of saline matter obtained by evaporating to dryness the distilled water in which an egg has been boiled, amounts, at an average, to about .32 grain. This saline residuum is strongly alkaline, and yields traces of animal matter, sulphuric acid, phosphoric acid, chlorine, an alkali, lime and magnesia, and carbonates of lime and magnesia; in short, of almost every principle existing in the egg. The carbonate of lime, however, is generally most abundant, and is obtained by evaporation in the form of a fine powder.

The shells of eggs have been analyzed by VAUQUELIN* and MERAT GUILLOT;† but these chemists seem to have over-rated the quantity of animal matter, and of phosphate of lime contained in them. When shells which had been dried in vacuo at 212°, were dissolved in dilute muriatic acid, the quantity of animal matter obtained was only about 2 per cent. while the quantity of phosphates of lime and of magnesia never amounted to quite 1 per cent.; the rest was carbonate of lime mixed with a little carbonate of magnesia. When burnt, egg-shells, as VAUQUELIN has observed, yield traces of sulphur and iron.

The *membrana putaminis*, on the supposition that the origi-

* Annales de Chimie, tom. 29 et 81.

† Ibid. tom. 34. It is probable that the different results obtained by these chemists depended, in a great degree, on the different mode in which the experiments were made. The phosphate of lime present in egg-shells is apparently connected with the animal matter, and when the latter is destroyed by combustion, the whole quantity present will of course be obtained. The quantity of animal matter present also, being in this mode of analysis necessarily estimated from the loss of weight occurring during the process, must appear greater than it ought to do, because part of this loss will obviously depend on the escape of water.

nal weight of the egg be 1000 grains, weighs, when dried in vacuo at 212°, about 2.35 grains ; and on being burnt, yields traces of phosphate of lime.

It may be observed here, that the great differences in the quantities of the earthy matter existing in the shells of different eggs, have rendered the average totally inapplicable in these experiments, as will be shown hereafter ; hence, a more detailed analysis of this part of the egg was deemed unnecessary.

Saline contents of the recent egg.

As my attention at present is chiefly confined to the fixed principles of the egg, I shall not here enter on a description of its immediate principles, which will be found sufficiently detailed in all the modern chemical treatises. The saline principles, however, particularly of the yelk, have been less minutely examined ; hence, it becomes necessary to relate the manner in which the following analyses were conducted ; and here it may be premised generally, that all the results were obtained by combustion ; and that the following observations are to be understood as applicable to the whole of the experiments subsequently related in this inquiry.

The *albumen* burns with difficulty, unless care be taken to remove the saline matter by frequent washings ; but if this point be attended to, the whole of the carbonaceous matter may be burnt off even in a covered crucible. In the subsequent experiments, the saline and earthy matters were removed from the crucible after combustion by distilled water ; a little ammonia was then added, and the whole permitted to remain at rest for twenty-four hours ; the clear solution containing the alkaline salts was now carefully poured off, and the insoluble residuum, consisting of the phosphate of lime and triple phos-

phate of magnesia and ammonia, after being washed with distilled water, was dried and weighed. The alkaline solution, together with the washings of the earthy phosphates, were then evaporated to dryness, and exposed to a low red heat; and the weight of the saline residuum being accurately noticed, the whole was again dissolved in distilled water. A few drops of nitric acid being now added to neutralize the excess of alkali present, nitrate of barytes was dropped into the solution as long as any precipitate fell. The precipitate was obtained by decanting off the solution as before, and, after being well washed, its weight ascertained: from this the quantity of sulphuric acid present was determined by calculation.* To the solution, thus freed from sulphuric acid, nitrate of barytes, and afterwards ammonia, were added. The phosphate of barytes thus obtained was collected, washed and weighed as before, and the quantity of phosphoric acid present obtained by calculation.† Nitric acid was again added in slight excess to the original solution, and nitrate of silver dropped into it as long as any precipitate fell; from the chloride of silver obtained, the quantity of chlorine present was estimated.‡ Lastly, the weights of the sulphuric and phosphoric acids and chlorine were added together, and their amount subtracted from the weight of the alkaline residuum formerly obtained by evaporation, the remainder, of course, indicated the quan-

* On the supposition that the weight of the atom of sulphuric acid is 50, and that of barytes 97.5, oxygen being 10.

† On the supposition that the weight of the atom of phosphoric acid is 35, that of oxygen being 10.

‡ On the supposition that the weight of the atom of chlorine is 45, and of silver 137.5, that of oxygen being 10.

tity of potash and soda,* and carbonates of potash and soda present. Finally, the proportion of the earthy phosphates to one another was determined, and the quantities of the bases and acid obtained by calculation.

The *yelk* of the egg is exceedingly difficult of combustion; and indeed without proper precautions cannot be burnt at all, on account of the large quantity of phosphorus it contains; which, by undergoing a partial combustion, forms a glassy coating that effectually excludes the contact of the air from the coal, and prevents its farther combustion. After a variety of attempts, the following were the two methods employed. The yelk of an egg which had been boiled hard, and dried by exposure to the air, was rubbed in a mortar with a quantity of bicarbonate of potash. The mixture was then introduced into a platina crucible and exposed to a strong red heat, till the flame had ceased to escape from a small hole in the lid. The crucible being now removed from the fire, its contents, when cold, were again pulverised in a mortar with nitre. This mixture was then introduced a little at a time into the covered crucible till the whole was burnt. To the residuum distilled water was added, which of course took up every thing but the earthy phosphates, which were separated and weighed, while the alkaline solution, like that before mentioned obtained from the albumen, was submitted to the action of the appropriate re-agents, and thus the quantities of the different acids present ascertained. In this manner every thing was determined, except the proportion of alkaline matter present; and to ascertain this, other experi-

* The quantity of soda equivalent to the sodium in union with the chlorine, was determined by calculation.

ments with different yelks were made, in which lime and nitrate of lime were substituted for the bicarbonate and nitrate of potash.

With respect to the modes in which the different fixed principles originally exist in the egg, it is very probable, as Berzelius has remarked, that the sulphuric acid obtained from albumen is a product of combustion, and exists in it naturally as sulphur. The same also appears to be the case, to a great extent at least, with respect to the phosphoric acid, especially that obtained from the yelk. The chlorine seems to exist originally in union with sodium, forming common salt. As to the earthy principles, Berzelius is of opinion that their metallic bases are probably to be considered as constituent principles of the primary animal compounds. These circumstances have induced me to state the quantities of the acids obtained separately from the bases.

It may be also remarked here, that as the following experiments were made almost entirely with the view of comparison only, my object was rather to conduct them in some general and uniform manner, than to enter into any very minute discriminations, which did not appear to be immediately necessary to my purpose. For this reason, the proportion of the potash to the soda, and the exact quantity of carbonic acid combined with them, were not attempted to be determined. With the same view also, the proportion of the lime to the magnesia, though ascertained, was not expressed, but the united weights of both introduced into the column. Lastly, every one acquainted with chemistry will perceive, that from the mode of operating, the weights of the different principles will be somewhat underrated.

The relative proportions of the saline principles of different eggs vary in some instances considerably.* The three following are selected from a variety of other analyses as examples: the weight of each egg being reduced for the sake of comparison to 1000 grains.

No. 1.†

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Albumen	.29	.45	.94	2.92	.30
Yelk -	.21	3.56	.39	.50	.68
Total -	.50	4.01	1.33	3.42	.98

No. 2.

Albumen	.15	.46	.93	2.93	.25
Yelk -	.06	3.50	.28	.27	.61
Total -	.21	3.96	1.21	3.20	.86

* The most remarkable variations occur in the quantities of the sulphuric acid and chlorine. The tables exhibit instances of these, but I have met with still more striking anomalies, for which I was unable to account. I have sometimes thought these differences, as well as some other singular ones observed with respect to the earthy matters, might be connected with the sex of the future bird, but as no proof of this could be obtained, the results have been suppressed, on the supposition that they arose from some error in conducting the experiment. The three analyses given may be considered as average results. It may be also observed, that besides the above principles, *iron* is met with in almost all products of combustion; and the quantity in the egg, as the process of incubation proceeds, apparently increases considerably; but it was found impossible to ascertain its quantity with any degree of precision.

† The numbers in this and the following tables were obtained by calculation. In general, I did not weigh nearer than $\frac{1}{40}$th of a grain, but as the substances weighed were compounds, it was thought that the calculations of their constituent principles might be safely carried to the second decimal figure.

No. 3.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Albumen	.18	.48	.87	2.72	.32
Yelk -	.19	4.00	.44	.51	.67
Total -	.37	4.48	1.31	3.23	.99

Although the consideration of the immediate principles of the egg does not fall within my present design, yet I cannot refrain from giving the following analysis of the yelk of the recent egg.

The egg from which the yelk had been taken, which is the subject of the following experiment, had been boiled hard in distilled water, and the yelk, in its moist state, was found to weigh 316.5 grains. It was then partially dried by exposure to the air for several weeks; and to remove the remainder of the water was reduced to powder, and exposed to a temperature of somewhat more than 212°. The total loss of weight was 170.2 grains, which was supposed to indicate the quantity of water present. The remainder was now digested repeatedly in alcohol of specific gravity .807, till that fluid came off colourless. The residuum was perfectly white and pulverulent, and possessed many of the properties of albumen; but it differed from that principle, by the large proportion of phosphorus it contained in some unknown state of combination. The alcoholic solution was of a deep yellow colour, and deposited crystals of a sebaceous matter, and a portion of a yellow semi-fluid oil. On distilling off the alcohol, the

oil was obtained in a separate state. On cooling, it became nearly solid, and weighed 91 grains. The albuminous principle above mentioned, weighed 55.3 grains. Hence this yelk consisted of

	Grains.
Water . .	170.2
*Albumen ? .	55.3
Yellow Oil. .	91.0
	316.5

But I have reason to believe that the proportions of these ingredients differ a little in different eggs.

Experiments on the egg at the end of the first week of incubation, or about the 8th day.

At the end of the first week, it has been found, on an average, that on the supposition that the egg originally weighed 1000 grains, it has now lost about 50 grains, and the weights of the constituent principles of two eggs in their moist state, were as follow:

	No. 1. Grains.	No. 2. Grains.
Unchanged albumen	232.8	247.1
Modified albumen	179.8	275.2
Liq. amnii, membranes, blood-vessels, &c.	97.0	
Animal	22.0	
Yelk	301.3	324.5
Shell and loss	167.1	153.2
	1000.0	1000.0

* This proportion of the albuminous principle does not differ much from that stated to exist in the yelk of the common fowl, by Mr. HATCHETT. Philos. Trans. vol. cvi, p. 308.

The consideration of the organization, &c. of the incubated egg, like that of its constituent principles, does not fall within my present design; yet, as some points connected with these subjects seem to be illustrated by the present inquiry, and as my experiments would be scarcely intelligible without them, I shall make a few brief remarks on the general phenomena presented by the different constituent principles of the egg at those periods at which it has been submitted to examination.

It has been remarked by many observers, that soon after the process of incubation has commenced, the yelk becomes more fluid than usual, and that as the liquor amnii increases, that portion of the albumen occupying the upper and larger end of the egg, begins to assume a peculiar appearance. In the present experiments (in which the egg was always previously boiled), the liquor amnii and portion of albumen in question, at the period now under consideration, exhibited somewhat the appearance of curds and whey. Nor did the analogy consist in mere appearance; for the curdy looking matter, which was of a yellow colour, and which I have termed *modified* albumen, resembled the curdy part of milk in its properties, so far as to contain intermixed with it an oily or butyraceous principle. A portion of this oily principle, on being separated and examined, was found to be soluble in alcohol, of a bright yellow colour: and in short, to possess all the properties of the yellow oil existing in the yelk. The yelk at this period, as before observed, has become more fluid, and appears larger and of a paler colour than natural. Haller, indeed, asserts, that it has not increased in weight, but the above table renders the reverse very probable. These appearances of the albumen and yelk have induced most

observers to believe that an interchange of principles takes place between them, while others seem to have mistaken the yellow modified albumen for the yelk itself. That an interchange of principles has taken place, at least under the above circumstances, there can be no doubt; yet the two are not indiscriminately mixed; for when the egg has been previously boiled, the yelk, though softer than natural, is nevertheless rendered of a firmer consistence than the modified albumen, and can thus be readily separated from it; there is, moreover, a distinct line of demarcation between them, arising, apparently, from the proper membrane of the yelk. Another argument in favour of the opinion of the intermixture of the albumen and yelk at this period, is derived from the following analyses of these constituent principles of the egg; from which it will be found, that the quantity of the saline matter is diminished in the albumen, and increased in the yelk. It is a singular and striking fact, however, that although the oily matter of the yelk has made its way to the albumen, very little of the phosphorus, which exists in such large quantities in the yelk, has been removed with it.

No. 1.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged albumen	.13	.27	.19	1.03	.18
Modified albumen, liquor amnii, animal, membranes, &c.	.08	.38	.45	1.17	.12
Yelk.	.09	4.03	.60	.80	.68
	.30	4.68	1.24	3.00	.98

No. 2.

	Sulphuric Acid.	Phospho-ric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Mag-nesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged Albumen	.18	.18	.24	1.50	.12
Modified albumen, liquor amnii, animal, membranes, &c.	.10	.25	.30	.70	.12
Yelk . .	.08	4.00	.56	.75	.67
	.36	4.43	1.10	2.95	.91

The following are the results of an analysis made two days later, or on the 10th day of incubation.

	Sulphuric Acid.	Phospho-ric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Mag-nesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged Albumen	.27	.14	.24	1.13	.12
Modified albumen, liquor amnii, animal, membranes, &c.	.08	.65	.68	1.36	.27
Yelk . . .	.05	3.35	.30	.62	.66
	.40	4.14	1.22	3.11	1.05

At this period the proportion of phosphorus is somewhat diminished in the yelk, and increased in the animal and its appendages. The chlorine and alkaline principles seem also to have diminished in the yelk, and to have increased a little in the albuminous portion.

How the above interchange of principles takes place between the albumen and yelk, does not appear to be distinctly understood. MAITRE JEAN, LEVEILLÉ, and others, suppose that it takes place through the chalazæ; and LEVEILLE has even pretended to demonstrate the tubular structure of one of the chalazæ. This tubular structure has been denied by some writers, and particularly by Dr. MACARTNEY,* who even appears to doubt the fact of the intermixture in question. After what has been said, however, there cannot, I think, be much doubt of the circumstance; and future inquirers on this interesting, but difficult subject, will do well to turn their attention to it.

Experiments on the egg at the end of the second week, or about the 15th day of incubation.

At the end of the second week of incubation, an egg has lost upon an average about 130 grains, on the supposition that its original weight was 1000 grains, and the weights of the constituent principles of two eggs were as follow:

	Grains.	Grains.
Unchanged Albumen . .	175.5	208.0
Liquor amnii, membranes, &c.	273.5	218.2
Animal	70.0	89.1
Yelk	250.7	248.0
Shell and Loss . . .	230.3	236.7
	1000.0	1000.0

At this period the animal has attained a considerable size, while the albumen has become diminished in a corresponding

* Article INCUBATION, in REES's Encyclopœdia.

degree. The albumen has also acquired a very firm consistence, especially when coagulated by heat. The liquor amnii has become more fluid, and the *modified* albumen, formerly mentioned, has very much diminished in quantity, or disappeared.* The yelk, which at the end of the first week seemed to have increased in bulk and fluidity, has now apparently acquired its original size and consistence. The following are the results of the analyses of the constituent principles of the above two eggs.

No. 1.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Unchanged albumen. -	.07	.22	.09	.73	.10
Liq. amnii, membranes, &c.	.06	.21	.71	.96	.08
Animal - - -	.06	.23	.09	.46	.27
Yelk - - -	.30	3.34	.16	.68	.69
	.49	4.00	1.05	2.83	1.14

No. 2.

Unchanged albumen. .	.11	.19	.23	.97	.09
Liq. amnii, membranes, &c.	.03	.20	.70	1.07	.08
Animal - - -	.06	.24	.07	.44	.28
Yelk - - -	.20	3.30	.10	.42	.70
	.40	3.93	1.10	2.90	1.15

* About this time HARVEY, and other observers, have noticed the appearance of a curdy or coagulated substance in the œsophagus, crop, stomach, and intestines of the animal. Is this a portion of the *modified* albumen above mentioned?

An egg, analyzed two days later, or on the 17th day of incubation, gave the following results:

No. 3.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	G ains.	Grains.
Liquor amnii, membranes, animal, &c.	.34	1.70	.68	2.40	1.10
Yelk	.10	2.50	.30	.56	.75
	.44	4.20	.98	2.96	1.85

At this period ossification, which, according to HALLER and others, begins about the 7th day, has made some progress. The yelk has parted with some of its phosphorus, which appears in the other principles of the egg; this is particularly observable in No. 3. The quantity of earthy matter has also increased, particularly in No. 3.

Experiments on the egg at the end of the third week, or at the full period of incubation.

At this period an egg has lost upon an average about 160 grains, on the supposition that its original weight was 1000 grains; and the weights of the constituent principles of two eggs in their moist state and without boiling, were as follow:

	Grains.	Grains.
Residuum of albumen, membranes, &c.	29 5	38.1
Animal	555.1	553.6
Yelk	167.7	151.3
Shell and loss	247.7	257.0
	1000.0	1000.0

At this period all the important changes of incubation are completed. The albumen has now disappeared, or is reduced to a few dried membranes and an earthy residuum (apparently consisting of the original earthy matter of the albumen which has remained unappropriated). The yelk is considerably reduced in size*, and is taken into the abdomen of the chick, while the animal has attained a weight nearly corresponding to the original weight of the albumen, added to that lost by the yelk, *minus* the total weight sustained by the egg during incubation. The alkaline matters and chlorine, which have been decreasing from the commencement of incubation, have now undergone farther diminution in quantity,† while *the earthy matters have increased in the most striking manner.* The other principles seem to have suffered very little change in quantity. The following are the results of the analyses of the above two eggs.

No. 1.

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Resid. of albumen, membranes, &c.	.04	.12	.09	.23	.12
Animal - -	.44	3.02	.55	2.26	2.58
Yelk - - -	.04	1.06	.03	.06	1.26
	.52	4.20	.67	2.55	3.96

* This has been denied or doubted by some writers, especially HALLER and Dr. MACARTNEY.

† We have seen that in boiling an egg, a portion of its saline matter makes its way through the shell into the water. Do the saline matters, in the present instance, escape with the water lost during incubation? An argument in favour of the supposition is, that the loss is chiefly confined to those salts already existing in the egg,

No. 2.

	Sulphuric Acid.	Phospho-ric Acid.	Chlorine.	Potash, Soda, and Carb. of Ditto.	Lime, Magnesia, and Carb. of Ditto.
	Grains.	Grains.	Grains.	Grains.	Grains.
Resid. of albumen, membranes, &c.	.03	.13	.09	.25	.12
Animal - -	.21	2.71	.68	2.12	2.60
Yelk - -	.02	1.23	.06	.03	1.10
	.26	4.07	.83	2.40	3.82

It may be proper to observe, that the above analyses are selected as the most perfect, from a variety of others made at each period, all of which confirm the results here given, though they differ, like those indeed given, in some subordinate particulars.*

These experiments, then, demonstrate, or render probable, the following circumstances.

1. That the relative weights of the constituent principles of different eggs vary very considerably.

2. That an egg loses about one-sixth of its weight during incubation, a quantity amounting to eight times as much as it loses in the same time under ordinary circumstances.

namely, the chlorine and alkaline matters. With respect to the use of these saline matters we know very little. Do they perform an office in the animal economy analogous to acid solutions in the galvanic battery?

* An interesting circumstance may be here mentioned, which I have never seen noticed by any writer on the present subject. At the end of the process of incubation, and for some time before, the animal is so situated in the egg, as, by its superior weight on one side, to cause the egg to assume such a position that the beak of the animal shall be uppermost, and consequently fully exposed to the air when it first makes its way through the shell.

3. That in the earlier stages of incubation, an interchange of principles takes place between the yelk and a portion of the albumen; that this interchange is confined on the part of the yelk to a little of its oily matter, which is found mixed with the above mentioned albumen; that this portion of albumen undergoes some remarkable changes, and is converted into a substance analogous in its appearance, as well as in some of its properties, to the curd of milk; and lastly, that a portion of the watery and saline portion of the albumen is found mixed with the yelk, which becomes thus apparently increased in size.

4. That as incubation proceeds, the saline and watery parts again quit the yelk, which is thus reduced to its original bulk; that in the last week of the process it undergoes still further diminution in weight, and loses the greater portion of its phosphorus, which is found in the animal converted into phosphoric acid, and in union with *lime*, constituting its *bony skeleton;* and lastly, that this lime does not originally exist in the recent egg, but is derived from some unknown source during the process of incubation.

These, and other interesting circumstances, arising out of the present inquiry, suggest some important hints respecting certain operations of the animal economy. They also serve to direct the attention of the microscopic inquirer to the investigation of points, which it is probably within his power to elucidate, but of which, at present, we are ignorant. This part of the subject, however, cannot be in abler hands than it is at present. To the distinguished physiologists, therefore, who have been recently engaged in the investigation, I willingly leave it, and shall conclude with a few remarks only

on the uses of the yelk, and the apparent generation of earthy matter.

Sir EVERARD HOME and Mr. HATCHETT have concluded, from their experiments, that the yelk is analogous to the milk of viviparous aimals, but more concentrated, and that its chief use is to afford a pabulum to the young animal during incubation.* This opinion, which is indeed as old as ARISTOTLE,† is corroborated in a striking manner by the present inquiry. Mr. HATCHETT has also made the important and curious remark, that in the ova of those tribes of animals, the embryos of which have bones, there is a portion of oily matter; and in those ova whose embryos consist entirely of soft parts, there is none. Hence it is concluded, that a certain portion of oil is necessary for the formation of bone. The present inquiry cannot be said to confirm, or invalidate, this remark, for although in the earlier stages of incubation, before ossification has commenced, a portion of the oil of the yelk is appropriated to the purposes of the economy of the animal, yet by far the greater portion of it remains; and some of it is even retained by the yelk till its final disappearance.‡ One great use of the yelk evidently is to fur-

* Philos. Trans. Vol. 106, p. 301, et seq.

† Ἡ μὲν οὖν ἀρχὴ τοῦ νεοττοῦ ἐστιν ἐκ τοῦ λευκοῦ, ἡ δὲ τροφὴ διὰ τοῦ ὀμφαλοῦ ἐκ τοῦ ὠχροῦ. Aristotelis de Animal. Hist. VI. 3. (Ed. Schneider.) PLINY makes the same remark. Ipsum animal ex albo liquore ovi corporatur. Cibus in luteo est. Hist. Nat. X. 53.

‡ I examined a chick on the 18th day after incubation. The yelk was now reduced to less than 2 grains, but it was of its original yellow colour, and of course contained oily matter. When burnt, it left traces of phosphate of lime. Dr. MACARTNEY attempts to show that the yelk does not pass into the intestine through the ductus vitello intestinalis, but is taken up by absorption; and an argument he adduces in support of this opinion, is, that the earthy matter is left

nish the phosphorus, entering as phosphoric acid, into the skeleton of the animal; but that the earthy portion of the bones is derived from the transmutation of oil into lime cannot, perhaps, be safely asserted in the present state of the inquiry.

With respect to the earthy matter found in the skeleton of the chick when it quits the shell, I think I can venture to assert, after the most patient and attentive investigation, that it does *not pre-exist in the recent egg;* certainly not, at least in any known state. The only possible sources, therefore, whence it can be derived, are from the shell, or transmutation from other principles. Whether it be actually derived from the shell, cannot be determined by chemistry; because, as we have seen, the shells of different eggs differ so much, that the application of averages is out of the question; and we are of course precluded from ascertaining the exact quantity of lime any particular shell originally contains. There are, however, very strong reasons for believing that the earthy matter is not derived from the shell. In the first place, the membrana putaminis never becomes vascular, and seems analogous to the epidermis; hence the lime of the shell, which is exterior to this membrane, is generally considered by physiologists as *extra-vascular;** it is therefore extremely difficult

behind in the yelk. In the present instance, however, the quantity of earthy matter was very minute: it had therefore disappeared, as well as the other principles of the yelk. When the chick is younger, the quantity of earthy matter is said to be much larger. HALLER asserts that the yelk disappears about the 16th day; and ARISTOTLE long ago remarked, that very little of it was left on the 10th day, after the chick had left the egg.

* See an Essay "on the connection between the vascular and extra-vascular parts of animals," by Sir A. CARLISLE. THOMSON's Annals, Vol. VI. p. 174.

to conceive how the earth in question can be introduced into the economy of the chick from this source, particularly during the last week of incubation, when a very large portion of the membranes are actually separated from the shell. Secondly, both the albumen and yelk contain, at the end of incubation, a considerable proportion of earthy matter (the yelk apparently more than it did originally); why is this not appropriated, in preference to that existing in the shell? In opposition to these arguments it will be doubtless stated, that the shell of the egg becomes brittle at the end of incubation, and appears to undergo, during that process, some other changes not at present understood. To which it may be answered, that this brittleness has been attributed to the separation of the membrana putaminis, and the exsiccation of the parts by so long an exposure to the heat necessary to the process of incubation; and in this manner all the *known* changes produced on the shell by incubation may perhaps be satisfactorily accounted for. Until, therefore, it be demonstrated that some other changes take place in the shell, I confess this argument does not seem to me to have much weight. I by no means wish however to be understood to assert, that the earth is *not* derived from the shell; because, in this case, the only alternative left me is to assert that it is formed by transmutation from other matter; an assertion, which I confess myself not bold enough to make in the present state of our knowledge, however strongly I may be inclined to believe that, within certain limits, this power is to be ranked among the capabilities of the vital energies.

XXIX. *On the Placenta.* *By Sir* EVERARD HOME, *Bart. V.P.R.S.*

Read June 27, 1822.

TILL the human ovum had been detected in the uterus, before it became attached, we could not have ascertained that the rudiments of the placenta were not formed at the same period with the chorion, and were, therefore, a part of the ovum belonging wholly to the female.

That fortunate discovery enables us now to determine that the placenta, and the changes in the chorion, which in some animals fit it to perform the functions of a placenta, depend as much upon the compound influence of the male and female, as the peculiarities of the embryo itself.

That the umbilical arteries had their origin from the branches of the aorta called iliacæ internæ; that these passed along the sides of the urinary bladder to the navel, where they composed the navel string, has been long known to anatomists; but in what manner the amnion was broken through, to allow these arteries to come at the chorion, has not even at this day been satisfactorily explained.

This must now be admitted to take place by the urinary bladder, on its first formation, bursting the amnion, in the same manner as the vesicle in the embryo of the bird bursts the membrane of the yelk, as is shown in the microscopical drawings annexed to a Paper I recently laid before the Society.

When it is considered that the office of the placenta is to supply the circulation of the blood in the embryo with ma-

terials for the growth of its different parts, and when we find so great a variety in the structure of placentas in different animals, it seems to follow that every genus of animals, in as much as it has a peculiar form, should have a peculiar placenta; and this, upon examination, is found to be the case.

The difference in the form of the placenta, which has hitherto had so little attention paid to it, is therefore to be considered as the means employed by nature to prevent the whole system respecting animals from being thrown into confusion, by preventing any two different genera from breeding together. We now see why this cannot take place, since a new form of placenta would be required intermediate between those of the two genera, for which there is no provision.

Upon the structure of the placenta or chorion must depend the period of utero-gestation. Where they are very vascular, it will be short, and where the reverse, very long. The human placenta is massy, thick, its arteries very large, and numerous; the utero-gestation is nine months. The placenta is wanting in the mare, there being only a very vascular chorion; the utero-gestation in that genus is eleven months. The utero-gestation of the elephant, according to Mr. CORSE, is twenty-two months; from which I am led to conclude there is no placenta.

The placenta would appear to be more or less in a perfect state, according to the care which is taken of the animal. The cow's utero-gestation at a mean is 284 days. The wild cow is stated to go 308, the longest period I believe respecting cows upon record.

This explains the latitude met with in utero-gestation, which is noticed in the Bulletin de Sciences, by the Philomatique Society in Paris, for the year 1797, by Mr. TESSIER.

In 160 cows, some calved in 241 days; 5 in 308; giving a latitude of 67 days

In 102 mares, 3 foaled in 311 days; 1 in 394 days; giving a latitude of 83 days.

In 15 sows, 1 littered in 109 days; 1 in 123 days; giving a latitude of 14 days.

In 139 rabbits, 1 produced its young in 26 days; 9 in 33 days; giving a latitude of 7 days.

As the human species has the form of placenta best fitted for the supply of blood to the fœtus, there appears to be less latitude than in other animals, at least in civilized society, where the nourishment of the mother is an object of the first attention; it may be otherwise in women living in a savage state.

Where the female of one species of animals breeds from the male of another, the utero-gestation of whose species is different, there appears to be no approximation in the time the hybrid is brought forth; but the longest period of the two is the time of such utero-gestation.

The mare, when covered by an ass, goes 11 months, her usual period. The Earl of MORTON's mare, covered by the quagga, went 339 days 19 hours.

The she ass, when covered by a horse, goes 11 months, although ten is her usual period.

The direct cause of parturition has never been satisfactorily explained; and the great latitude there is in the utero-gestation of individuals of the same species, which has just been shown, makes it evident, that the cause must be something immediately connected with the complete formation of the fœtus. Upon this subject I am induced to hazard the following observations.

The lungs are the last parts of the embryo that are completely formed. As soon as this happens, the blood that circulates through the lungs must greatly diminish the supply of blood to the placenta; the consequence of which will be, that the small terminal arteries of the fœtal portion will become contracted; which cannot happen to any great degree without producing more or less of a separation of the placenta and chorion, and this will be followed by the expulsion of the young. That very slight disturbance at this period brings on labour, is well known, from the number of children prematurely born, that are with difficulty kept alive till the circulation through the lungs becomes complete: they are called blue children.

I shall not carry these observations farther at present, and shall conclude them with a specimen of a new mode of classing animals, upon the principles laid down respecting the difference in structure of the placenta.

It will have the advantage, that the characters are fixed, and therefore the arrangement will never require to be changed: and there is a circumstance in its favour—it places the human species in an order separate from all inferior animals, the place undoubtedly assigned for mankind by the Almighty Creator.

Class I.

includes all animals in which the ovum becomes attached to the womb of the mother.

Ova with adherent chorions.

7 Orders.

1. Lobulated placenta. *Vide* Plates XLII. and XLIII.
   One genus; one species; Man.

2. Subdivided placenta. *Vide* Plates XLIV. and XLV.
   One genus; all the Monkey tribe.
3. Belted placenta. *Vide* Plate XLVI.
   Two genera; first belt thick; Lion tribe.
   second belt thin; Dog tribe.
4. Placenta with many divisions. *Vide* Plate XLVII.
   One genus, with five divisions; Hare tribe.
   The others not yet known.
5. Cotyloid placenta.
   Genera 5.
   1. simple; Hedgehog.
   2. plane; Mole.
   3. thick; Bat.
   4. pedunculated; Rat.
   5. pediculated; Guinea-pig.
6. Numerous cotyledons.
   Genera 4.
   1. the terminal arteries, with lateral branches; Cow.
   2. the terminal arteries filiform; Deer.
   3. the terminal arteries villous; Sheep.
   4. the terminal arteries like shag; Goat.
7. Chorion without placenta.
   Genera 4.
   1. projecting plexuses; two species.
      Thick; Mare.
      Thin; Ass.
   2. stellated; Hog.
   3. vascular membrane; Camel.
   4. tufted; Whale.

EXPLANATION OF THE PLATES.

PLATE XLII.

The fœtal surface of the human placenta. The amnion turned a little inwards.

PLATE XLIII.

The uterine surface of the human placenta.

PLATE XLIV.

The fœtal surface of the placenta of the Monkey. The whole surface covered by the amnion.

PLATE XLV.

The uterine surface of the placenta of the Monkey.

PLATE XLVI.

Fig. 1. A fœtus of the Cat, inclosed in its membranes, to show the uterine surface of its circular placenta.

Fig. 2. The fœtal surface of the placenta of the Rabbit, divided into five lobes.

Fig. 3. The uterine surface of the same placenta.

PLATE XLVII.

Shows the cotyloid placentæ.

Fig. 1. Of the Hedge-hog.

Fig. 2. Of the Rat.

Fig. 3. Of the Guinea-pig.

Plate XLVIII.

Fig. 1. The uterine portion of the cotyledon of the Cow.

Fig. 2. The uterine surface of the fœtal portion of the cotyledon.

Fig. 3. The terminating vessels of the fœtal portion of the cotyledon of the Deer.

Fig. 4. The terminating vessels of the fœtal portion of the cotyledon of the Sheep.

Fig. 5. The terminating vessels of the fœtal portion of the cotyledon of the Goat.

XXX. *Of the geographical situation of the Three Presidencies, Calcutta, Madras, and Bombay, in the East Indies. By* J. Goldingham, *Esq. F. R. S.*

Read June 27, 1822.

In the present advanced state of knowledge it may be useless to dwell upon the importance to navigation, as well as to general geography, of correct information relative to the latitudes and longitudes of the principal places on the surface of our globe. The ease with which the situation of a place *on* the meridian is obtained, for general purposes, is well known, and the comparative difficulty of ascertaining the distance, east or west, *from* a given meridian is equally so, particularly where that meridian is a quarter of the globe distant, which is the case as relates to India. Having, however, one point correctly determined, the situations of others, at moderate distances from it, may be come at with greater facility; either by chronometers, by correspondent observations, or, where places are on the same continent, by actual survey.

One of the best methods of determining the position of a point, thus distant from the first meridian, is by eclipses of the satellites of Jupiter. Correspondent observations of eclipses of the sun, of the moon, or of occultations, happen but seldom, and the method by the moon's transit requires, that the position of that luminary should be correctly set down in the Tables; or, in the case of correspondent transits, that the instruments at both places should be most accurately

placed in the meridian, and the transits taken with the least possible error of observation; as only a very small error in the Tables, or in the observed place of the moon, may produce a considerable one in the result. But eclipses of the satellites of Jupiter occur often, and correspondent ones with those taken at Greenwich, are not *very* unfrequent, even in this distant part of the globe. The observations taken at Greenwich also show the difference or error of the Tables, and consequently, the error of the longitude deduced from them. Errors also which may arise from a difference in the powers of the telescopes, and in the eyes of observers, as well as from a general difference in the state of the atmosphere, may be counterbalanced by taking a series of these eclipses, consisting of immersions as well as emersions.

I shall, therefore, for the present at least, as regards the longitude of Madras, draw a conclusion from these eclipses; a very long catalogue of which has been taken at the Madras Observatory. So numerous, indeed, are these observations, that the longitude of Madras, which I may give at a future time, by other methods, may perhaps be considered more as corroborating *that* now deduced, than as furnishing information for correcting it.

We may now, however, notice a result obtained from lunar observations. Of these, about 800 have been taken at various times since the year 1787, with different sextants; and reduced to the Observatory, give its longitude 2′ 55,″5 more than by the satellites. This will furnish us with a correction for numerous observations of this description taken at Bombay, when we come to treat of the longitude of that place.

The first set of the following eclipses is composed of ob-

servations taken at different places in India; the differences of meridians between which and the Observatory are correctly known, either from correspondent eclipses, chronometers, or by survey. These having been taken with different telescopes, and by different observers, and also at a distance from the Observatory, may be considered as less valuable than those observed there, with the same description of telescope, and under every favourable circumstance.

The second result is from eclipses taken at Madras, with different telescopes, at two or three different points, and reduced to the Observatory. These may also be considered of less value than the third result, which is drawn from eclipses taken at the Observatory with the same description of telescope, and under favourable circumstances. I have, therefore, in drawing the conclusion, considered a mean of the first and second results as about equal in value to the third, and have combined them accordingly. The fourth result is from correspondent eclipses, which I considered of equal value with the other three results. This relates to the first series of eclipses from 1787 to 1801.

In the second series, containing eclipses taken between the years 1803 and 1816, the first result was deduced from observations taken at the Observatory with the same description of telescope, and is therefore of equal value with the mean of the two first results, and also with the third of the other series; and the results have been combined accordingly. The second result of the second series is obtained from correspondent eclipses; and, like the former, by observations of this description, is considered of equal value with the mean of all the results found by correcting the Tables.* The differ-

* In drawing the conclusion, I have combined *all* the correspondent eclipses.

ences, applied to the longitude, found by the Tables in the Ephemeris were obtained by eclipses taken at Greenwich, as near the time as possible that each eclipse was observed at Madras.

The following are the observations and results. It may be proper to state, that some additional observations were at first included in these Tables; that a mean was taken; and, when any longitude differed more than 30 seconds from the mean, it was rejected; and it is only these eclipses which were within 30 seconds of the general mean that are here included. The observations being so numerous, enabled me to make this selection. The general result in both cases is however very nearly the same, as is commonly the case; there being found as many rejected observations giving a longitude too great as too little.*

* In finding the difference of the Tables, reference has been made to the circumstance under which the Greenwich observation *nearest* the time was taken, and its value in consequence; as well as to other observations taken *about* the time.

Longitude of the Madras Observatory by the Eclipses of the Satellites of Jupiter, from 1787 to 1801, corrected for the difference of the Tables from the Observations taken at Greenwich at or about the time of each Eclipse.

Day.	Place.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris.	Longitude in Time.	Difference of the Tables.	Corrected Longitude.	Difference of Longitude to the Observatory.	Longitude of the Observatory.
			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.	m. s.	h. m. s.
1787.									
Feb. 9	Calcutta.	1 E	8 33 13	2 39 36	5 53 37	+ 14	5 53 51	32 25,2	5 21 25,8
16	——	1 E	10 29 26	4 35 34	5 53 52	+ 14	5 54 6	32 5,2	5 21 40,8
22	——	2 E	8 26 48	2 33 48	5 53 00	+ 1,20	5 54 20	32 28	5 21 54,8
25	——	1 E	6 55 22	1 1 23	5 53 59	+ 14	5 54 13	32 28	5 21 47,8
1789.									
Dec. 17	Coringa.	1 I	17 6 58,5	11 38 41	5 28 17,5	+ 27	5 28 44,5	8 19,7	5 20 24,8
19	——	1 I	11 34 23,2	6 6 9	5 28 14,2	+ 28	5 28 42,2	8 19,7	5 20 22,5
29	——	2 I	13 28 53	8 0 17	5 28 36	+ 12	5 28 48	8 19,7	5 20 28,3
1790.									
Jan. 2	——	1 I	15 14 41,4	9 46 9	5 28 32,4	+ 32	5 29 4,4	8 19,7	5 20 44,7
23	Masulipatam.	2 I	10 15 49	4 51 46	5 24 3	+ 47	5 21 50	3 38,3	5 21 10,3
Feb. 1	——	1 I	17 4 4	11 40 3	5 24 1	+ 39	5 24 50	3 38,3	5 20 0,3
26	——	1 E	13 59 46	8 35 24	5 24 22	+ 48	5 25 10	3 38,3	5 21 30,3
1791.									
Jan. 5	Bombay.	1 I	17 30 54,3	12 40 6	4 50 48,3	+ 30	5 51 18,3	29 38,4	5 20 56,7
7	——	1 I	11 58 2	7 7 42	4 50 20	+ 32	4 50 52	29 38,4	5 20 30,4
Feb. 20	——	1 I	17 41 35	12 50 52	4 50 43	+ 24	4 51 7	29 38,4	5 20 45,4
22	——	1 I	12 10 3	7 19 32	4 50 31	+ 26	4 50 57	29 38,4	5 20 35,4
March 1	——	1 I	14 5 10,5	9 14 43	4 50 27,5	+ 35	4 51 2,5	29 38,4	5 20 40,9
8	——	1 I	16 0 31,5	11 10 21	4 50 10,5	+ 30	4 50 40,5	29 38,4	5 20 18,9
24	——	1 E	16 35 26	11 43 39	4 51 47	+ 19	4 52 6	29 38,4	5 21 44,4
April 4	——	1 E	7 29 30	2 38 16	4 51 14	+ 39	4 51 53	29 38,4	5 21 31,4
11	——	1 E	9 25 39	4 34 87	4 21 2	+ 34	4 51 36	29 38,4	5 21 14,4
18	——	1 E	11 22 31	6 30 47	4 51 44	+36, 3	4 52 20,3	29 38,4	5 21 58,7
25	——	1 E	13 17 51,2	8 26 45	4 51 6,2	+ 43	4 51 49,2	29 38,4	5 21 27,6
27	——	1 E	7 46 41,2	2 55 41	4 51 0,2	+ 43	4 51 43,2	29 38,4	5 21 21,6
May 27	——	1 E	9 54 2,8	5 3 18	4 50 44,8	+ 51	4 51 35,8	29 38,4	5 21 14,6
1789.									
Jan. 29	Tranquebar.	2 E	14 21 10	9 1 26	5 19 44	— 12	5 19 32	+1 34	5 21 6
31	——	1 E	10 40 54	5 22 5	5 18 49	+ 1,10	5 19 59	+1 34	5 21 33
Feb. 14	——	1 E	14 29 56	9 10 25	5 19 31	+12, 5	5 19 43,5	1 34	5 21 17,5
23	——	1 E	10 54 17	5 34 36	5 19 41	+12, 5	5 19 33,5	1 34	5 21 27,5
May 28	——	1 E	6 33 4	1 13 20	5 19 44	+ 20	5 20 4	+1 34	5 21 38
1790.									
Jan. 23	——	2 I	10 10 12	4 51 46	5 18 26	+ 1, 6	5 19 32	1 34	5 21 6
25	——	1 I	15 5 52	9 47 23	5 18 29	+38,	5 19 7	1 34	5 20 41
30	——	2 I	12 43 29	7 25 39	5 17 50	+ 1,6	5 18 56	1 34	5 20 30
1787.									
Nov. 19	Madras.	2 I	8 18 54	2 58 8	5 20 46	+ 1, 7	5 21 53	1 2	5 21 51,8
Dec. 21	——	2 E	10 35 57	5 14 3	5 21 54	— 27	5 21 27	1 2	5 21 25,8
28	——	2 E	13 10 10	7 48 32	5 21 38	— 27	5 21 11	1 2	5 21 9,8
1788.									
Jan. 27	——	1 E	13 1 14	7 41 6	5 20 8	+ 50	5 20 58	1 2	5 20 56,8
Feb. 12	——	1 E	11 19 3	5 58 43	5 20 20	+ 50	5 21 10	1 2	5 21 8,8
23	——	2 E	10 1 56	4 42 0	5 19 56	+ 1,14	5 21 10	1 2	5 21 8,8
March 22	——	1 E	10 1 47,8	4 40 53	5 20 54,8	+ 12	5 21 6,8	1 2	5 21 5,6
31	——	1 E	6 28 44,2	1 7 54	5 20 50,2	+ 12	5 21 2,2	1 2	5 21 1
April 23	——	1 E	6 49 43,5	1 28 48	5 20 55,5	— 3	5 20 52,5	1 2	5 20 51,3

Day.	Place.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris.	Longitude in Time.	Difference of the Tables.	Corrected Longitude.	Difference of Longitude to the Observatory.	Longitude of the Observatory.
1788.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.	s.	h. m. s.
Nov. 5	Madras.	1 I	15 44 47,1	10 24 20	5 20 27,1	+ 52	5 21 19,1	5,4	5 21 15,7
14	—	1 I	12 5 39,3	6 45 9	5 20 30,3	+ 52	5 21 22,3	5,4	5 21 16,9
28	—	1 I	15 48 47,6	10 28 10	5 20 37,6	+ 1, 0	5 21 37,6	5,4	5 21 32,2
30	—	1 I	10 16 37,8	4 55 52	5 20 45,8	+ 1, 0	5 21 45,8	5,4	5 21 40,40
1789.									
April 3	—	1 E	9 37 3	4 16 27	5 20 36	+ 27	5 21 3	1	5 21 2
26	—	1 E	9 56 17,6	4 35 36	5 20 41,6	+ 20	5 21 1,6	1,2	5 21 0,4
1790.									
Jan. 25	—	1 I	15 7 51,7	9 47 23	5 20 28,7	+ 38	5 21 6,7	9,4	5 20 57,3
30	—	2 I	12 46 18,1	7 25 39	5 20 39,1	+ 1, 6	5 21 45,1	9,4	5 21 35,7
Feb. 3	—	1 I	11 28 48,9	6 8 19	5 20 29,9	+ 40	5 21 9,9	1,2	5 21 8,7
10	—	1 I	13 22 13,3	8 1 58	5 20 15,3	+ 40	5 20 55,3	1,2	5 20 54,1
26	—	1 E	13 55 29	8 35 24	5 20 5	+ 48	5 20 53	1,2	5 20 51,8
28	—	1 E	8 24 25,2	3 4 17	5 20 8,2	+ 48	5 20 56,2	1,2	5 20 55
March 14	—	1 E	12 16 39,3	6 56 19	5 20 20,3	+ 32	5 20 52,3	1,2	5 20 51,1
21	—	1 E	14 13 00	8 52 45	5 20 15	+ 52	5 21 7	1,2	5 21 5,8
April 6	—	1 E	12 35 42,8	7 15 20	5 20 22,8	+ 42	5 21 10,8	1,2	5 21 9,6
8	—	1 E	7 4 30,9	1 44 27	5 20 3,9	+ 48	5 20 51,9	1,2	5 20 50,7
15	—	1 E	9 0 55,49	3 40 50	5 20 5,4	+ 49	5 20 54,4	1,2	5 20 53,2
22	—	1 E	10 57 13,4	5 36 58	5 20 15,4	+ 49	5 21 4,4	1,2	5 21 3,2
1792.									
March 19	—	1 I	15 46 24,3	10 25 55	5 20 29,3	+ 54	5 21 23,3	0,2	5 21 23,5
21	—	1 I	10 15 37	4 54 57	5 20 40	+ 54	5 21 34	0,2	5 21 34,2
28	—	1 I	12 11 21,2	6 51 7	5 20 14,2	+ 48	5 21 2,2	+ 0,2	5 21 2,4
May 13	—	1 E	14 52 5,5	9 31 40	5 20 25,5	+ 30	5 20 55,5	+ 0,2	5 20 55,7

The Coringa, Masulipatam, and Tranquebar observations were taken by the late Mr. Topping: the Calcutta observations also by the late Mr. Topping: the Bombay observations by myself.

At the Madras Observatory.

Day.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris.	Longitude in Time.	Difference of the Tables.	Corrected Longitude.
1793.		h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
March 24	1 I	13 7 11	7 46 44	5 20 27	+ 24	5 20 51
31	1 I	15 2 57	9 42 32	5 20 25	+ 24	5 20 49
April 7	1 I	16 58 46,4	11 38 19	5 20 27,4	+ 50	5 21 17,4
9	1 I	11 27 22,9	6 7 16	5 26 6,9	+ 50	5 20 56,9
16	1 I	13 3 29,7	8 2 58	5 20 31,7	+ 50	5 21 21,7
May 1	2 I	13 12 46,2	7 53 9	5 19 37,2	+1 34	5 21 11,2

In the Madras observations which follow, sometimes three observers have taken the eclipse, sometimes two; but all the telescopes have the same power, and are exactly of the same construction, having been made by Dollond at one and the same time.

The two assistants at the Observatory are Bramins: the head assistant is named Senvassachary, and the second Verdachary.

Eclipses from 1794 *to* 1801, *with the same description of Telescope.*

Day.	Place.	Satellites.	Apparent Time. Observed at Madras.	Apparent Time. Per Ephemeris	Longitude in time.	Difference of the Tables.	Longitude of the Observatory.
			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
1794.							
May 5	Madras Observatory.	1 I	14 38 47	9 18 53	5 19 54	+ 48	5 20 42
12	——	1 I	16 33 14,3	11 13 2	5 20 12,3	+ 1,14	5 21 26,3
21	——	1 I	12 55 24,5	7 35 11	5 20 13,5	+ 1,14	5 21 27,5
28	——	1 I	14 48 46	9 28 37	5 20 19	+ 1,16	5 21 25
30	——	1 I	9 16 39,6	3 56 54	5 19 45,6	+ 1,16	5 21 16
June 4	——	1 I	16 41 41,3	11 21 44	5 19 57,3	+ 1,18	5 21 15,3
6	——	1 I	11 9 53	5 49 59	5 19 54	+ 1,18	5 21 12
10	——	2 I	16 52 37	11 32 26	5 20 11	+ 1, 2	5 21 13
1795.							
Sept. 4	——	1 E	9 57 44,5	4 37 10	5 20 34,5	+ 17	5 20 51,5
11	——	1 E	11 55 37,5	6 35 11	5 20 26,5	+23, 6	5 20 50,1
1796.							
July 22	——	1 I	10 30 14,9	5 9 28	5 20 46,9	+20, 4	5 21 7,3
29	——	1 I	12 24 15	7 3 33	5 20 42	+ 20	5 21 2
Sept. 13	——	1 E	15 14 3,6	9 53 8	5 20 55,6	+10, 2	5 21 5,8
1797.							
Oct. 18	——	1 E	15 27 59,5	10 7 7	5 20 52,5	+39, 9	5 21 32,4
20	——	1 E	9 56 39,3	4 36 9	5 20 30,3	+ 40	5 21 10,3
Nov. 3	——	1 E	13 47 55,7	8 27 20	5 20 35,7	+37, 7	5 21 13,4
5	——	1 E	8 16 26,7	2 56 6	5 20 20,7	+41, 4	5 21 2,1
12	——	1 E	10 11 14,3	4 50 49	5 20 25,3	+49, 2	5 21 14,5
1798.							
Jan. 29	——	1 E	6 56 37,3	1 35 51	5 20 46,3	+14, 6	5 21 0,9
Feb. 21	——	1 E	7 13 4,8	1 52 21	5 20 43,8	+ 8, 1	5 20 51,9
Oct. 7	——	1 I	13 1 48	7 40 24	5 21 24	16	5 21 8
16	——	1 I	9 26 12,3	4 4 53	5 21 19,3	12, 5	5 21 6,8
—	——	2 I	11 29 32	6 8 50	5 20 42	+ 3, 4	5 20 45,4
23	——	1 I	11 21 24,2	6 0 2	5 21 22,2	— 15	5 21 7,2
30	——	1 I	13 15 57,8	7 54 41	5 21 16,8	— 17	5 20 59,8
Nov. 17	——	1 E	8 7 14	2 46 45	5 20 29	+ 35	5 21 4
—	——	2 E	13 45 17,8	8 22 26	5 22 51,8	1,18	5 21 33,8
Dec. 12	——	2 E	10 47 12,5	5 25 34	5 21 38,5	—20, 9	5 21 17,6
1799.							
Jan. 16	——	1 E	12 1 19,6	6 40 36	5 20 43,6	+ 36	5 21 19,6
18	——	1 E	6 29 15,2	1 8 56	5 20 19,2	+ 35	5 20 54,2
25	——	1 E	8 23 10,5	3 2 42	5 20 28,5	+ 30	5 20 58,5
Oct. 3	——	1 I	14 34 57,6	9 14 1	5 20 56,6	+ 8, 2	5 21 4,8
10	——	1 I	16 30 18,7	11 9 26	5 20 52,7	— 0, 7	5 20 52
Sept. 15	——	2 I	13 32 42,6	8 14 34	5 21 8,6	+ 21	5 21 9,6
1800.							
Feb. 6	——	1 E	8 4 35,5	2 44 8	5 20 27,5	+ 37	5 21 4,5
13	——	1 E	10 0 4,8	4 39 12	5 20 52,8	+ 30	5 21 22,8
March 24	——	1 E	8 42 25	3 21 38	5 20 47	0, 0	5 20 47
Nov. 23	——	1 I	12 37 19,2	7 16 37	5 20 42,2	+30, 6	5 21 12,8

Correspondent Eclipses of the Satellites of Jupiter, from the year 1787 to 1800.

Day.	Place.	Satellites.	Apparent Time. Observed Place.	Apparent Time. At Greenwich.	Longitude in Time.	Difference of Longitude to the Observatory.	Longitude of the Observatory.
			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
1787.							
Dec. 21	Madras.	2 E	10 35 57	5 14 30	5 21 27	1,2	5 21 25,8
1789.							
Dec. 19	Coringa	1 I	17 6 58,5	11 38 14	5 28 44,5	8 21,7	5 20 22,8
1790.							
Jan. 25	Madras.	1 I	15 7 51,7	9 46 45	5 21 6,7	1,2	5 21 5,5
Feb. 26	Masulipatam.	1 E	13 59 46	8 34 36	5 25 10	3 39,7	5 21 30,3
March 3	Madras.	2 E	15 20 25,7	9 58 41	5 21 44,7	— 1,2	5 21 43,5
8	——	3 E	12 27 45,5	7 6 39	5 21 6,5	— 1,2	5 21 5,3
21	——	1 E	14 13 00	8 51 53	5 21 7	1,2	5 21 5,8
1791.							
March 1	Bombay.	1 I	14 5 10,5	9 14 8	4 51 2,5	+29 38,3	5 20 40,8
——	——	3 I	15 22 12	10 31 15	4 50 57	29 38,3	5 20 35,3
24	——	1 E	16 35 26	11 43 20	4 52 6	29 38,3	5 21 44,3
April 25	——	1 E	13 17 51,2	8 26 2	4 51 49,2	29 38,3	5 21 27,5
1792.							
March 19	Madras.	1 I	15 46 24,3	10 25 19	5 21 5,3		5 21 5,3
April 11	——	1 I	16 3 35	10 42 55	5 21 00		5 21 00
1793.							
May 8	Masulipatam.	2 I	15 50 11,4	10 25 36	5 20 35,4	3 39,7	5 20 55,7
1794.							
June 10	{ Madras Observatory. }	2 I	16 52 37	11 31 24			5 21 13
1796.							
Sept. 13.	——	1 E	15 14 3,6	9 52 57,8			5 21 5,7
1797.							
Oct. 18	——	1 E	15 27 59,5	10 6 25			5 21 34,5
1798.							
Nov. 15	——	1 E	13 39 55,5	8 17 53,1			5 22 2,4
Dec. 12	——	2 E	10 47 12,5	5 25 54,8			5 21 17,7
1799.							
Jan. 16	——	1 E	12 1 19,6	6 40 02			5 21 19,4
Oct. 10	——	1 I	16 30 18,7	11 9 26,7			5 20 52

RESULTS

By the First and Second Satellites, observed at different places in India, but reduced to the Madras Observatory. Longitude by

Immersions.	Emersions.
h. m. s.	h. m. s.
5 20 24, 8	5 21 25, 8
20 22, 5	21 40, 8
20 28, 3	21 54, 8
20 24, 7	21 47, 8
21 11, 7	21 31, 7
21 1, 7	21 44, 4
20 56, 7	21 31, 4
20 30, 4	21 14, 4
20 45, 4	21 58, 7
20 35, 4	21 27, 6
20 40, 9	21 21, 6
20 18, 9	21 14, 2
21 6, 0	21 6, 0
20 41, 0	21 33, 0
20 30, 0	21 17, 5
	21 27, 5
5 20 41,23	21 38, 0
	21 31,48
	20 41,23
Mean	5 21 6,35 E

RESULTS

First and Second Satellites observed at Madras. Longitude by

Immersions.	Emersions.
h. m. s.	h. m. s.
5 21 51, 8	5 21 25, 8
21 13, 7	21 9, 8
21 16, 9	20 56, 8
21 32, 2	21 8, 8
21 40, 4	21 8, 8
20 57, 3	21 5, 6
21 35, 7	21 1, 0
21 8, 7	20 51, 3
20 54, 1	21 2, 0
21 23, 5	20 0, 4
21 34, 2	20 51, 8
21 2, 4	20 55, 0
20 51, 0	20 51, 1
20 49, 0	20 5, 8
21 17, 4	21 9, 6
20 56, 9	20 50, 7
20 21, 7	20 53, 2
21 11, 2	21 3, 2
	20 55, 7
5 21 15,45	
	5 21 1,39
	21 15,45
Mean	5 21 8,42

RESULTS

By Eclipses from 1794 to 1801. First and Second Satellites observed at Madras, with the same Telescope. Longitude by

Immersions.	Emersions.
h. m. s.	h. m. s.
5 20 42, 0	5 20 51, 5
21 26, 3	20 50, 1
21 27, 5	21 5, 8
21 25, 0	21 32, 4
21 1, 6	21 10, 3
21 15, 3	21 13, 4
21 12, 0	21 2, 1
21 13, 0	21 14, 5
21 7, 3	21 0, 9
21 2, 0	20 51, 9
21 8, 0	21 4, 0
21 6, 8	21 33, 8
20 45, 4	21 17, 6
21 7, 2	21 19, 6
20 59, 8	20 54, 2
21 29, 6	20 58, 5
21 4, 8	21 4, 5
20 52, 0	21 22, 8
21 12, 8	20 47, 0
5 21 8,34	5 21 7, 1
	21 8,34
Mean	5 21 7,72

RESULTS.

Correspondent Eclipses from 1787 to 1800. First, Second, and Third Satellites. Longitude by

Immersions.	Emersions.
h. m. s.	h. m. s.
5 20 22, 8	5 21 25, 8
21 5, 5	21 30, 3
20 40, 8	21 43, 5
20 35, 3	21 5, 3
21 5, 3	21 5, 8
21 0, 0	21 44, 3
20 55, 7	21 27, 5
21 13, 0	21 5, 8
21 52, 0	21 34, 5
	22 2, 4
	21 17, 7
	21 19, 4

Longitude of the Observatory by correcting the Tables, from 1803 to 1815.

Date.	Satellites.	Immersions or Emersions.	Mean Time by the Nautical Almanac.	Mean Time observed at Madras.	Longitude of Madras by Tables.	Difference of the Tables.	Longitude of Madras.
1803.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
Feb. 1	1	Im.	6 1 52	11 23 14,53	5 21 22,43	0 18,0	5 21 4,4
18	2	Im.	8 5 34	13 27 1,39	5 21 27,39	0 28,0	5 20 59,4
March 15	2	Im.	5 5 33	10 26 35,37	5 21 2,37	0 28,0	5 20 34,4
April 2	2	Em.	2 11 22	7 32 18,48	5 20 56,48	+0 24,0	5 21 20,5
20	1	Em.	5 6 25	10 27 32,30	5 21 7,30	—0 21,0	5 20 46,3
27	1	Em.	7 0 45	12 21 46,86	5 21 1,66	—0 21,0	5 20 40,6
May 13	1	Em.	5 18 17	10 39 19,66	5 21 2,66		
20	1	Em.	7 12 47	12 33 55,26	5 21 8,26		
July 7	1	Em.	2 6 1	7 27 20,01	5 21 19,01	+0 6,0	5 21 25,0
1804.							
Jan. 12	1	Im.	8 44 19	14 5 7,60	5 20 48,60		
26	1	Im.	12 31 8	17 15 10,15	5 21 2,15		5 21 6,7
April 22	1	Em.	7 52 54	13 14 13,19	5 21 19,19	+0 4,5	5 21 24,7
24	1	Em.	2 21 23	7 42 42,67	5 21 19,67		5 21 24,2
May 1	1	Em.	4 15 41	9 36 40,83	5 20 59,33		5 21 4,3
4	2	Em.	2 45 28	8 6 51,90	5 21 23,90	+0 9,5	5 21 33,4
8	1	Em.	6 10 3	11 30 41,90	5 20 38,90		5 20 48,4
1805.							
March 10	1	Im.	8 15 31	13 36 34,63	5 21 3,63		
23	2	Im.	10 33 21	15 55 11,66	5 21 50,66		
24	1	Im.	12 2 28	17 23 25,33	5 20 57,33		
26	1	Im.	6 30 54	11 51 50,60	5 20 56,60		
April 2	1	Im.	8 24 32	13 45 25,56	5 20 53,56		
18	1	Im.	6 40 25	12 1 18,63	5 20 53,63		
25	1	Im.	8 34 21	13 55 11,33	5 20 50,33		
May 4	1	Im.	4 57 0	10 18 17,34	5 21 17,34		
July 21	1	Em.	4 7 35	9 28 4,35	5 20 29,35		
28	1	Em.	6 2 42	11 23 24,26	5 20 42,26		
1806.							
Feb. 18	1	Im.	11 6 43	16 27 34,86	5 20 51,86		
April 14	1	Im.	7 44 31	13 5 0,71	5 20 29,71		
18	2	Im.	9 0 9	14 21 47,09	5 21 38,09		
21	1	Im.	9 37 57	14 58 50,76	5 20 53,76		
May 14	1	Im.	9 47 11	15 7 53,87	5 20 42,87	+0 34,0	5 21 16,9
27	2	Im.	11 6 52	16 28 55,43	5 22 3,43		
Sept. 4	2	Em.	1 46 52	7 7 43,26	5 20 51,26	+0 12,0	5 21 3,2
Oct. 6	2	Em.	1 28 30	6 49 16,34	5 20 46,34	—0 22,0	5 21 8,3
1807.							
May 3	1	Im.	9 11 7	14 32 19,06	5 21 12,06		
10	1	Im.	11 4 31	16 25 40,30	5 21 9,30		
26	1	Im.	9 19 47	14 41 3,23	5 21 16,23		
June 2	1	Im.	11 13 22	16 34 32,96	5 21 10,96		
Aug. 11	2	Em.	6 53 31	12 14 41,88	5 21 10,88	+0 15,5	5 21 26,4
29	2	Em.	1 21 19	6 42 12, 8	5 20 53, 8		5 21 9,3
30	1	Em.	1 20 20	6 41 8, 6	5 20 48, 6	+0 15,5	5 21 4,1
Oct. 6	1	Em.	5 27 36	10 48 26, 1	5 20 50, 1	+0 8,0	5 20 58,1
22	1	Em.	3 48 10	9 9 2,	5 20 50, 0	—0 3,2	5 20 48,8
Nov. 7	1	Em.	2 8 52	7 29 40,	5 20 47, 0	+0 1,4	5 20 48,4
1808.							
May 21	1	Im.	10 53 36	16 14 40,18	5 21 4,18		
June 13	1	Im.	11 2 31	16 23 43,57	5 21 12,57		
22	2	Im.	12 5 34	17 26 29,67	5 20 55,67		
29	1	Im.	9 18 9	14 39 22,54	5 21 13,54		

Date.	Satellites.	Immersions or Emersions.	Mean Time by the Nautical Almanac.	Mean Time observed at Madras.	Longitude of Madras by Tables.	Difference of the Tables.	Longitude of Madras.
1808.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
Sept. 17	1	Em.	2 57 48	8 18 15,16	5 20 27,16	+0 24,5	5 20 51,7
24							
Oct. 26	1	Em.	1 32 53	6 54 0,83	5 21 7,83	−0 12,0	5 20 55,8
Nov. 9	1	Em.	5 24 37	10 45 39,37	5 21 2,37	. . .	5 21 2,4
1809.							
Aug. 26	1	Im.	9 38 18	14 59 22,31	5 21 4,31	−0 19,1	5 20 45,2
Oct. 22	1	Em.	3 1 58	8 23 1,32	5 21 3,32	. . .	5 21 3,3
Nov. 5	1	Em.	6 52 11	12 13 14,46	5 21 3,46	+0 2,0	5 21 5,5
7	1	Em.	1 20 58	6 42 2,04	5 21 4,04	+0 3,5	5 21 7,5
14	1	Em.	3 16 19	8 37 26,98	5 21 7,98	+0 4,8	5 21 12,8
Dec. 4	2	Em.	5 8 22	10 28 37,28	5 20 15,28	+0 50,3	5 21 5,5
7	1	Em.	3 32 10	8 53 9,75	5 20 59,75	+0 2,0	5 20 59,9
23	1	Em.	1 52 51	7 14 1,27	5 21 10,27	+0 1,5	5 21 11,8
29	2	Em.	2 15 46	7 36 15,20	5 20 29,20		
30	1	Em.	3 48 50	9 10 4,25	5 21 14,25	+0 1,0	5 21 15,2
1810.							
Jan. 30	2	Em.	1 58 10	7 18 42,93	5 20 32,93		
March 3	2	Em.	1 39 14	7 0 5,25	5 20 51,25	+0 12,4	5 21 3,7
June 10	2	Im.	11 44 27	17 5 20,07	5 20 53,07		
July 30	1	Im.	11 13 50	16 34 48,95	5 20 58,95		
Oct. 9	1	Im.	6 9 57	11 30 55,63	5 20 58,63	0 6,7	5 20 51,9
Nov. 21	2	Em.	2 15 16	7 35 49,93	5 20 33,93		
28	2	Em.	4 52 56	10 13 17,72	5 20 21,72		
Dec. 3	1	Em.	5 2 44	10 23 31,81	5 20 47,81	+0 23,8	5 21 11,6
1811.							
Jan. 4	1	Em.	1 41 2	7 1 30,77	5 20 28,77	+1 0,3	5 21 28,8
11	1	Em.	3 36 33	8 57 7,86	5 20 34,86	+1 0,3	5 21 35,2
24	2	Em.	1 52 15	7 12 43,12	5 20 28,12	+1 6,0	5 21 34,1
31	2	Em.	4 29 15	9 49 19,04	5 20 4,04	+1 6,0	5 21 10
Feb. 25	2	Em.	1 37 40	6 57 57,54	5 20 17,54	+1 6,0	5 21 23,5
March 29	2	Em.	1 20 10	6 39 58,61	5 19 48,61	+1 6,0	5 20 54,6
Oct. 12	1	Im.	9 37 4	14 58 22,99	5 21 18,99	+0 2,4	5 21 21,4
Nov. 11	2	Im.	9 44 25	15 6 54,36	5 22 29,36	−0 56,1	5 21 33,3
	1	Im.	11 39 6	17 0 15,28	5 21 9,28	+0 5,6	5 21 14,9
Dec. 6	1	Im.	6 16 55	11 37 43,76	5 20 48,76		5 20 54,3
	2	Im.	6 52 49	12 14 23,57	5 21 34,57		
31	2	Em.	6 46 28	12 7 17,03	5 20 49,03	+0 5,6	5 20 54,6
1812.							
Jan. 14	1	Em.	6 56 38	12 17 6,94	5 20 28,94	+0 19,0	5 20 47,9
21	1	Em.	8 51 23	14 11 57,31	5 20 34,31	+0 24,0	5 20 54,3
Feb. 1	2	Em.	6 35 36	11 55 30,48	5 19 54,48		
6	1	Em.	7 9 59	12 30 33,79	5 20 34,79	+ 32,4	5 21 7,2
22	1	Em.	5 29 11	10 49 44,04	5 20 33,04		5 21 5,4
26	2	Em.	3 47 9	9 7 9,23	5 20 0,23		
March 2	1	Em.	1 53 17	7 13 47,50	5 20 30,50		5 20 47,3
16	1	Em.	5 44 6	11 4 46,49	5 20 40,49	+ 16,8	5 20 57,3
25	1	Em.	2 8 26	7 29 28,86	5 21 2,86		5 21 19,7
Oct. 7	1	Im.	10 57 17	16 19 9,65	5 21 52,65		
30	1	Im.	11 6 0	16 27 5,50	5 21 5,50		
Nov. 4	2	Im.	8 42 5	14 3 59,53	5 21 54,53	−4 6,8	5 21 7,7
11	2	Im.	11 16 57	16 38 46,31	5 21 49,31		5 21 2,5
Dec. 15	1	Im.	11 22 43	16 43 50,24	5 21 7,24		5 21 6,1
17	1	Im.	5 51 3	11 12 9,32	5 21 6,32	− 1,1	5 21 5,2
31	1	Im.	9 38 4	14 59 12,46	5 21 8,46		5 21 7,4

Date.	Satellites.	Immersions or Emersions.	Mean Time by the Nautical Almanac.	Mean Time observed at Madras.	Longitude of Madras by Tables.	Difference of Tables.	Longitude of Madras.
1813.			h. m. s.	h. m. s.	h. m. s.	m. s.	h. m. s.
Jan. 2	1	Im.	4 6 30	9 27 13,81	5 20 43,81		
7	2	Im.	8 2 42	13 23 46,62	5 21 4,62		
	1	Im.	11 31 42	16 52 36,75	5 20 54,75		
25	2	Em.	5 27 49	10 48 42,74	5 20 53,74		
Feb. 1	2	Em.	8 4 53	13 25 26,74	5 20 33,74		
10	1	Em.	4 47 55	10 8 56,35	5 21 1,35	+ 8,5	5 21 9, 8
19	2	Em.	2 38 41	7 59 6,12	5 20 25, 1	+ 11,9	5 20 37, 0
26	1	Em.	3 5 9	8 26 26,55	5 21 7, 5		5 21 16, 0
March 5	1	Em.	4 59 53	10 20 37,67	5 20 44, 6	+ 8,5	5 20 53, 1
12	1	Em.	6 54 34	12 15 21,18	5 20 47, 2		5 20 55, 7
21	1	Em.	3 17 58	8 39 15,40	5 21 17, 4	+ 8,5	5 21 25,9
23	2	Em.	2 26 47	7 47 29,87	5 20 42,87	+ 11,9	5 20 54,8
30	2	Em.	5 4 11	10 25 32,22	5 21 21, 2	+ 11,9	5 21 33, 1
April 24	2	Im.	2 14 45	7 35 41,48	5 25 56, 4	+ 11,9	5 21 8, 3
29	1	Em.	1 50 9	7 11 1,16	5 20 52, 1	+ 8,5	5 21 0, 6
May 22	1	Em.	2 4 2	7 25 22,66	5 21 20, 6	+ 8,5	5 21 29, 1
26	2	Em.	1 59 5	7 19 57,82	5 20 52, 8	+ 11,9	5 21 4, 7
Oct. 26	1	Im.	12 14 6	7 35 28,47	5 21 22, 4	— 20,9	5 21 1, 5
Dec. 20	1	Im.	8 53 23	14 14 56,75	5 21 33, 7	— 22,2	5 21 11, 5
1814.							
Jan. 1	2	Im.	6 46 56	12 8 56,42	5 22 0, 4	— 50,8	5 21 0, 6
19	1	Im.	10 55 16	16 16 32,23	5 21 16, 2	— 15,6	5 21 0, 6
28	1	Im.	7 17 12	12 38 36,01	5 21 24, 0		5 21 8, 4
Feb. 2	2	Im.	6 23 34	11 45 37,82	5 22 3, 8	— 59,5	5 21 4, 3
4	1	Im.	9 10 49	14 31 59,37	5 21 10, 3		5 21 22, 3
6	1	Im.	3 39 12	9 0 10,68	5 20 58, 6	+ 12,0	5 21 10, 6
13	1	Em.	5 32 56	10 54 6,43	5 20 10, 4		5 21 22, 4
March 10	1	Em.	2 25 54	7 46 46,83	5 20 52, 8	+ 12,0	5 21 4, 8
17	1	Em.	4 19 58	9 41 10,77	5 21 12, 7		5 21 24, 7
24	1	Em.	6 14 9	11 35 39,80	5 21 30, 8	+ 12,0	5 21 42, 8
31	2	Em.	6 0 27	11 21 8,95	5 20 41, 9		
April 2	1	Em.	2 37 1	7 58 20,97	5 21 19,97	— 0,6	5 21 18, 4
9	1	Em.	4 31 24	9 53 0,91	5 21 36, 9		5 21 36, 3
May 2	1	Em.	4 43 36	10 4 57,03	5 21 21, 0	— 0,6	5 21 20, 4
	2	Em.	4 48 33	11 10 6,13	5 21 33, 0	. . .	5 21 32, 5
June 10	2	Em.	3 14 12	8 35 10,97	5 20 59, 0	— 0,6	5 20 58, 4
Nov. 7	1	Im.	11 29 44	6 50 48,62	5 21 4, 6		
1815.							
Jan. 31	1	Im.	10 11 51	15 33 4,95	5 21 13, 9		5 20 56, 6
Feb. 7	1	Im.	12 5 20	17 26 34,74	5 21 14, 7	— 17,3	5 20 57, 4
9	1	Im.	6 33 46	11 54 59,62	5 21 13, 6		5 20 56, 3
10	2	Im.	10 15 53	15 37 59,42	5 22 6, 4	— 1 9,7	5 20 56, 7
25	1	Im.	4 49 19	10 10 28,43	5 21 9, 4	— 17,3	5 20 52, 1
28	2	Im.	4 40 42	10 2 46,48	5 22 4, 4	1 9,7	5 20 54, 7
March 4	1	Im.	6 43 2	12 4 12,23	5 21 10, 2	17,3	5 20 52, 9
14	2	Im.	9 49 39	15 11 41,74	5 22 2, 7	1 9,7	5 20 53, 0
Apri 3	1	Em.	10 57 53	16 19 38,57	5 21 45, 5	27,6	5 21 17, 9
12	1	Em.	7 20 26	12 42 1,54	5 21 35, 5	27,6	5 21 7, 9
April 14	1	Em.	1 49 0	7 10 18,98	5 21 19, 0	27,6	5 20 51, 4
19	2	Em.	1 18 54	6 40 37,22	5 21 43, 2	1 3,0	5 20 40, 2
28	1	Em.	5 37 27	10 58 56,64	5 21 29, 6	27,6	5 21 2, 0
May 5	1	Em.	7 31 48	12 53 26,69	5 21 38, 6		5 21 11,11

The eclipses from 1805 to 1811, were observed during my absence in England; Captain WARREN, of His Majesty's 33d regiment, acting for me.

By correspondent Eclipses of the Satellites of Jupiter at Madras and Greenwich, from 1803 to 1815.

Date.	Satellites.	Immersions or Emersions.	Mean Time at Madras.	Mean Time at Greenwich.	Longitude.
1810.			h. m. s.	h. m. s.	
Aug. 22	1	Im.	16 44 2,80	11 22 47,5	5 21 15, 3
1811.					
Nov. 20	1	Im.	13 22 0,47	8 0 57,3	5 21 3,17
1812.					
Nov. 22	1	Im.	16 35 23,14	11 14 3,5	5 21 19,64
1813.					
March 12	1	Em.	12 15 21,18	6 54 12,3	5 21 8,88
1814.					
Feb. 4	1	Im.	14 31 59,37	9 10 53,4	5 21 5,97
April 7	2	Em.	13 58 34,63	8 37 6,8	5 21 27,83
1815.					
Feb. 7	1	Im.	17 26 34,74	12 5 37,3	5 20 57,44
March 14	2	Im.	15 11 41,74	9 50 48,7	5 20 53,04
April 3	1	Em.	16 19 38,57	10 58 3,2	5 21 35,37

RESULTS

By the First Satellite.

Immersions. h.	m.	s.	Emersions. h.	m.	s.
5	21	4,4	5	20	46, 3
	21	6,7		20	40, 6
	21	16,9		20	25, 0
	20	45,2		21	24, 7
	20	51,9		21	24, 2
	21	21,4		21	4, 3
	21	14,9		20	48, 4
	20	54,3		21	4, 1
	21	6,1		20	58, 1
	21	5,2		20	48, 8
	21	7,4		20	48, 4
	21	1,5		20	51, 7
	21	11,5		20	55, 8
	21	0,6		21	2, 4
	21	8,4		21	3, 3
	21	22,3		21	5, 5
	21	10,6		21	7, 5
	21	22,4		21	12, 8
	20	56,6		20	59, 9
	20	57,4		21	11, 8
	20	56,3		21	15, 2
	21	52,1		21	11, 6
	20	52,9		21	28, 2
Mean 5	21	3,50		21	35, 2
				20	47, 9
				20	54, 3
				21	7, 2
				21	5, 4
				20	47, 3
				20	57, 3
				21	19, 7
				21	9, 8
				21	16, 0
				21	53, 1
				20	55, 7
				20	25, 9
				20	0, 6
				20	29, 1
				21	4, 8
				21	24, 7
				21	18, 4
				21	36, 3
				21	20, 4
				20	58, 4
				20	17, 9
				21	7, 9
				20	51, 4
				20	2, 0
				21	11, 0
			5	21	6,86
			5	21	3,50
			Mean 5	21	5,18

By the Second Satellite.

Immersions. h.	m.	s.	Emersions. h.	m.	s.
5	20	59, 4	5	21	20, 5
	20	34, 4		21	33, 4
	21	33, 3		21	3, 2
	21	7, 7		21	8, 3
	21	2, 5		24	26, 4
	21	0, 6		21	9, 3
	21	4, 3		21	5, 5
	20	56, 7		21	3, 7
	20	54, 7		21	34, 1
	20	53, 0		21	10, 0
5	21	0 67		21	23, 5
				20	54, 6
				20	54, 6
				20	37, 0
				20	54, 8
				21	33, 1
				21	8, 3
				21	4, 7
				21	32, 5
				20	40, 2
			5	21	9,88
			5	21	0,67
			Mean 5	21	5, 3

RESULTS

By the correspondent Eclipses from 1803 *to* 1816.

h.	m.	s.
5	21	15,30
	21	3,17
	21	19,64
	21	8,88
	21	5,97
	21	27,83
	20	57,44
	20	53,04
	21	35,37

Correspondent Eclipses from 1787 *to* 1816.

Immersions.

h.	m.	s.	
5	20	22, 8	
	21	5, 5	
	20	40, 8	
	20	35, 3	
	21	5, 3	
	21	0, 0	
	20	55, 7	
	21	13,	
	20	52,	
	21	15, 3	
	21	3,17	
	21	19,64	
	21	5,97	
	20	57,44	
	20	53,04	
5	20	57,66	Mean.
	21	26,28	Emersions.
	21	11,97	Mean.

Emersions.

h.	m.	s.	
5	21	25, 8	
	21	30, 3	
	21	43, 5	
	21	5, 3	
	21	5, 8	
	21	44, 3	
	21	27, 5	
	21	5, 7	
	21	34, 5	
	22	2, 4	
	21	17, 7	
	21	19, 4	
	21	8,88	
	21	27,83	
	21	35,37	
5	21	26,28	Mean.

Longitude of the Madras Observatory from the whole of the foregoing Observations. By correcting the Tables:

First and Second Satellites.

	h. m. s.		h. m. s.
Eclipses reduced to Madras,	5 21 6,35		
Observed at Madras, but not with the same telescope,	5 21 8,42	Mean	5 21 7,77
Observed at Madras to 1802, with the same description of telescope, - - - - -			5 21 7,72
From 1802 to 1815, with telescopes of the same powers, - - -			5 21 5,24
Mean by correcting the tables, - - -			5 21 6,78
By correspondent observations at Greenwich, from 1787 to 1816, - - - - - -			5 21 11,97
Mean, or longitude of the Observatory, - - - - -			5 21 9, 4
		East of Greenwich	80 17 21

Fort St. George Church-steeple is 2′ 21″ east of the Observatory; the longitude of the Steeple is therefore 80° 19′ 42″ east.

The longitude of the lunar observations before alluded to, about 800 in number, taken between the years 1787 and 1792* at different parts of Madras and at Coringa, and reduced to the Observatory, is 80° 20′ 16″,5 east; and therefore, according to the eclipses, 2′ 55″,5 too great.

* By the late Honourable W. PETRIE, Esq. the late Mr. TOPPING, and myself: the Coringa observations by Mr. TOPPING.

I shall now proceed to give some information respecting the latitude of the Observatory. The height of the pale at Madras being only 13 degrees, the method by circumpolar stars cannot be used; and the latitude has been found by meridianal observations of the sun and stars north and south of the zenith, taken with the sextant, a circular instrument of 18 inches diameter by TROUGHTON, and the zenith sector used in Colonel LAMBTON's Survey.

The results were as follow:

Latitude of the Madras Observatory by observations of stars with the sextant, - -		13° 4′ 8″,606
With the circular instrument, stars north and south of the zenith, -	13° 4′ 11″,894	
Second set, - - -	13 4 6,770	13 4 9,332
Stars near the zenith, - - -		13 4 7,917
Correspondent observations of the sun at Greenwich, - - - - -		13 4 11,163
Observations of the sun, - - -		13 4 5,363
Mean by the circular instrument and sextant, - - - - -		13 4 8,476
By observations with the zenith sector, stars north and south of the zenith, - - -	13 4 11,95	
Sun, - - - -	4 5,15	13 4 8,55
Mean latitude by Mr. GOLDINGHAM's observations, - - - - -		13 4 8,513

During my absence in England, I find the zenith sector was again brought to the Observatory, there being some doubt, as it would seem, of the correctness of the foregoing

conclusion.* The following are the results of the observations, which are very numerous.

By Captain WARREN's observations with the zenith sector.

Latitude, stars north and south of the zenith,

Table II. of the Records, -	13° 4′ 15″,074	
Table III. - - -	4 13 ,717	° ′ ″
Mean by stars - - -		13 4 14,395
by the sun - - - -		4 5,483
Mean latitude by Captain WARREN's Observations - - -		13 4 9,939
Mean latitude by both; Mr. GOLDINGHAM with the circular instrument and sextant - - -		13 4 8,479
Zenith sector, Stars - - -		11,950
Sun - - -		5,150
Captain WARREN's, zenith sector, Stars - - - - -		14,395
Sun - - - - -		5,483
Mean latitude† -		13 4 9,1N.

It would therefore appear that very little (if any) additional light had been thrown upon the subject by the latter obser-

* Owing, probably, to the difference between the latitude by the sun, and that by the stars; a difference, however, much greater in the latter observations than in the others; and, as I have observed, not easily accounted for. I hope, however, that the Observatory will be furnished with a large circle, which, besides enabling us to obtain other valuable information, may lead to a discovery of the cause of this difference.

† This result is found from about 700 observations.

vations; those formerly taken,* differing from the mean of the whole little more than half a second. The mean of the whole, 13° 4′ 9″,1,† may therefore be considered, for the present at least, the latitude of the Observatory.

It will be observed, that the meridianal observations of the sun give a different result from those of the stars. In mine, this difference is about 4 seconds less than by the stars; in the second series, the difference is nearly 9 seconds also less than by the stars; a difference not easily accounted for. It is curious however to remark, that the correspondent meridianal zenith distances of the sun at Greenwich, give a result *greater* than the mean latitude by the sun 5″,85; and the same elements are used in both cases, with the exception of declination.

Of the Longitude‡ of Calcutta.

By a series of correspondent eclipses of the satellites of Jupiter, taken in Fort William, by the late Lieutenant-Colonel COLEBROOKE, of the Bengal Establishment; the telescope at the Observatory, and that used at Calcutta, being in all

* The observations being so numerous, in the conclusions now drawn from mine, the method of selection used with the eclipses has been adopted, viz. by taking a mean of the whole, and then rejecting those results which differ more than the power of the instrument would seem to warrant: this, with the sextant, I have considered 10″, with the circular instrument 8″, and with the zenith sector 4″. The latitude formerly deduced stood thus: mean of the observations with the zenith sector 13° 4′ 8″,55; with the circular instrument 13° 4′ 8″,40; with the circular instrument and sextant 13° 4′ 8″,5; mean 13° 4′ 8″,48 or not one-tenth of a second different from the result now deduced by these observations.

† Fort St. George Church Steeple is 36″ N. of the Observatory. Latitude of the Steeple is therefore 13° 4′ 45″ N.

‡ According to RENNELL's Memoir, the Longitude by the Honourable THOMAS HOWE, was 88° 33′; by mean of four observers, 88° 27′ 45″.

respects alike, the difference of longitude was found to be 8° 6′ 18″.

Longitude of the Madras Observatory	80° 17′ 21″
Calcutta (Fort William) E. of the Observatory . . .	8 6 18
Longitude of Fort William* . .	88 23 39 E.

Of the Longitude of Bombay.

In the year 1791, being at Bombay, on my way from England to Madras, and aware that great doubts existed as to the longitude of that important commercial station,† I proposed taking some observations, while detained there for a passage to this coast, with the view of assisting in the determination of the question; and accordingly commenced observing the eclipses of the satellites of Jupiter, and a series of lunar observations; taking also at the same time a sufficient number of meridianal observations for determining the latitude. The results were as follow:

By the mean of about 160 lunar observations with a sextant by Troughton, the longitude of the place of observation at Bombay was 72° 57′ 39″ E.; and by the mean of 180 lunar observations with a sextant, having Ramsden's name on it, the longitude of the place was 72° 57′ 55″. The mean of both was 72° 57′ 47″.

But it would appear from the lunar observations taken at

* The latitude of Calcutta is considered 22° 33′ N.

† Mr. Howe's longitude, 72° 38′, appearing at the time to be considered the most correct; but Captain Huddart had placed it more than a quarter of a degree farther to the eastward.

Madras, that the tables about that period gave 2′ 55″,5 too much. If, therefore, this quantity be subtracted from the result, the longitude will be only 72° 54′ 52″ E.

More than 30 eclipses of the satellites of Jupiter, immersions as well as emersions, were also observed; and by allowing for the difference of the tables at that period by comparisons with Greenwich observations, the longitude of the place of observations, by the mean of the first and second satellites, was 72° 53′ 26″.

Also, by a very good chronometer, the rate of which I found before leaving Bombay, and after my arrival at Madras* (having been about 16 days on the passage), the difference of the longitude between the place of observation at Bombay and the Madras Observatory is . 7° 24′ 12″

This gives for the longitude of the former 7° 53′ 9″

The longitude of Bombay (place of observation) by the lunar observations corrected . .	72	54	52
By eclipses of the satellites of Jupiter, Tables corrected	72	53	26
By the difference of longitude by the chronometer	72	53	9
Mean	72	53	49

But the place of observation was 54 seconds of a degree west of Bombay Church, and 13 east of the Light House; therefore the longitude of the Church, by these observations, is 72° 54′ 43″, and of the Light House, 72° 53′ 36″ east of Greenwich.†

* Rate on quitting Bombay + 40″,43
On my arrival at Madras + 40, 34.

† The difference of meridians between Madras and Bombay Churches, according to these deductions, is 7° 24′ 59″.

The latitude was found by 32 meridianal observations of the sun and stars, north and south of the zenith, taken with the two sextants, and an artificial horizon. The height of the thermometer and that of the barometer was noted at the time of each observation, and the correction on this account was applied to the refraction. The declinations were also corrected for aberration, &c. and the results were:

By 16 observations with TROUGHTON's instrument	18° 57′ 43″,5
By 16 observations with RAMSDEN's .	18 57 43 ,8
Mean latitude	18 57 44 N.

The place of observation was 1′ 37″ north of the Church, and 3′ 19″* north of the Light House. The latitude of the Church is therefore 18° 56′ 7″, and of the Light House 18° 54′ 25″ N.

It may be useful to remark upon a difference with RAMSDEN's sextant, in the results by the objects north, and by those south of the zenith in observing for the latitude; and also the difference in the results by the lunar observations east and west of the moon. The instrument was most carefully examined, and the error regularly found every day by measuring the sun's diameter; yet, notwithstanding, the following differences were in the results:

In the observations for the latitude, the object being North of the zenith . .	18° 58′ 11″
South of the zenith . . .	18 57 16,6
Difference †	0 55

* A survey was made for ascertaining this, as well as for the difference of longitude.

† From this difference in the results for the latitude, a difference of more than half a degree might be looked for in those of the longitude, as we find to be the case.

In the observations for the longitude, the object

being East of the ☽	73°	15′	4″,4
West	72	40	46 ,2
Difference		34	18 ,2

The sextant by TROUGHTON, in the observations for the latitude, gave only a difference of four seconds between the results by the objects north, and those south of the zenith; and about three minutes in the observations for the longitude. Whether the differences in the results by the other sextant arose from an error in the total, or from what other cause, is not easy to determine. The instrument appeared perfect; but these results, however, show the necessity of observing objects both north and south of the zenith for the latitude; and also objects east and west of the moon for the longitude. The mean of the results thus obtained will be correct; the objects on one side, giving a longitude as much greater, as those on the other side give one as much less, than the truth. In consequence, we find that the mean latitude by RAMSDEN's instrument is only a few tenths of a second different from that by TROUGHTON's; while the mean longitude is only about sixteen seconds of a degree different.

J. GOLDINGHAM.

Madras,
6th December, 1819.

XXXI. *Of the difference of longitudes found by chronometer, and by correspondent eclipses of the satellites of Jupiter; with some supplementary information relative to Madras, Bombay, and Canton; as also the latitude and longitude of Point de Galle and the Friar's Hood.* By J. GOLDINGHAM, *Esq. F. R. S.*

Read June 27, 1822.

Of the difference of longitude found by chronometer, and by correspondent eclipses of the satellites of Jupiter.

IN my former Paper, I mentioned these methods of finding the longitude, after the distance of one point from a first and distant meridian had been correctly established, and at the same time carried both into practice; the former, in deducing the longitude of Bombay, and the latter, in the operation for the longitude of Calcutta; and I bring forward the following observations by these methods, to show what may be effected in practice by either, where the instruments are good, and the necessary precautions have been taken. The observations at Masulipatam were taken by the late Mr. TOPPING, those at the Observatory, by myself. The telescopes at both places were the same in construction and magnifying power.*

* Masulipatam is not two days sail from Madras in the S. W. monsoon.

Madras Observatory and Masulipatam Flag Staff.

By correspondent eclipses of the satellites of Jupiter.

	Satellites.	Em. or Im.	D. Long to Masulipatam Flag Staff.
1794	1	E	0° 52′ 6″
	1	E	0 48 22,5
1795	2	I	1 0 40,7
	1	I	1 3 0,7
	1	I	0 50 5,2
	2	I*	0 46 53,2
	1	I	0 50 45,7
	1	E	0 57 23,2
	2	E	0 59 38,2
	1	E	0
	1	E	0 57 17,2
	1	E	0 54 41,2
	2	E	1 1 33,7
Mean			0 55 12
Observatory			80 17 21
Masulipatam Flag Staff			81 12 33

By Chronometer.

	Difference of Meridians.
By the chronometers of ARNOLD's, in the year 1793	0° 55′ 43″
In the year 1794, by 2 chronometers of ARNOLD's	0 55 60
In the year 1795, by 1 chronometer of ARNOLD's	0 53 54
Mean	0 54 54
Observatory	80 17 21

Longitude of Masulipatam Flag Staff by chronometer	81° 12′ 15″
Longitude of Masulipatam Flag Staff by the eclipses	81 12 33
By the chronometer . . .	81 12 15
Mean	81 12 24 E

Here we find a wonderful agreement by the two methods; and the longitude of *Masulipatam*, a point of importance on the coast, may be considered correctly determined.

The following information may be deemed supplementary to that given in my former paper on the Geographical situation of the three Presidencies, and it is hoped will be found useful to navigators.

At Madras, the Fort Flag Staff is about 1″,6 north, and 2 east of the Church Steeple. This gives for the latitude of the Flag Staff 13° 4′ 47″ north, and longitude 80° 19′ 44″ E. As navigators generally take their departure from the Flag Staff, I have deemed it proper to give its situation, as well as that of the Steeple, given in the former paper.

The tide on the coast about Madras seldom rises more than 3 feet; and it is high water on the Syzigies, by my observations, at $9^h\,25^m$. The variation of the compass towards the end of 1792, on the coast, about a degree to the northward of Madras, was 1° 3′ east, by numerous observations.

At Bombay, the time of high water on the Syzigies, at the Dock Head, from several observations which I made, was $11^h\,32^m$. The pilots generally allow $11^h\,\frac{1}{2}$ as the time. The greatest rise of the tide at the Dock Head was 18 feet. This

happened at the springs near the vernal equinox of 1791; and except from particular combinations, it is never known to rise higher: as I was informed, the medium rise of the springs is about $15\frac{1}{2}$ feet. The variation of the compass in the beginning of the year 1791, by the mean of many observations, I found to be $42' 59''$, or $43'$ west.

During the passage from Bombay to Madras, I had an opportunity of ascertaining the latitude and longitude of Point de Galle, and of the Friar's Hood; and as the chronometer performed so well, it is to be regretted we did not see other places on the way. Point de Galle is, however, an important position to have accurately determined, and in the year 1791, the longitude deduced from different observations varied from $80° 1' 30''$ to $80° 22'$.

On the 8th of September we saw Point de Galle Flag Staff. Three bearings of it were taken with the azimuth compass; one when it was E. $8° 24'$ N.; a second when it was N. $28° 38'$ E.; and the third when it was due north. The time when each bearing was taken was correctly noted; and a base, measured by the run of the ship, corrected for current, was used for finding the distances, which, at the time the first bearing was taken, was 5,5 miles, and at the time the second was taken 3,7. The ship being in the longitude of Point de Galle at the time the third bearing was taken, no distance was necessary. Altitudes for finding the longitude by the chronometer were observed about half an hour before the first bearing was taken, which was at $5^h 45'$ p.m. apparent time. These altitudes give for the longitude of the ship at $5^h 10'$ p.m., (by mean of two days observations after my arrival at Madras, and allowing $80° 19' 42''$ as the longitude of the

Church) 80° 9′ 6″, the latitude of the ship being at that time 6° 2′ 4″ N.; the difference of latitude and difference of longitude made by the ship to the times the bearings were taken, being of latitude 2′ 4″ S. and 4′ 25″ S.; of longitude 2′ 27″ E. 6′ 6″ E. and 8′ 3″ E.

The latitude and longitude of Point de Galle Flag Staff from these observations were

	Latitude.	Longitude.
By the first bearing	6° 0′ 47″ N.	80° 16′ 59″
Second .	6 0 58	80 16 57
Third .	. . .	80 17 9*
	6 0 50	80 17 2 E.

Three bearings of the Friar's Hood were also taken on the 10th of September with an azimuth compass: one when we were nearly in the latitude, and another when in the longitude of the Hood. The same care was taken in finding the distances, &c.; and the latitude of the Hood was found to be 7° 29′ 35″ N.; longitude 81° 36′ 3″ $\frac{1}{4}$ E.

It will no doubt be concluded, that where so much attention was paid to have the correct bearings and distances, that the meridianal observation for the latitude, and the altitudes for the longitude, were taken with a sextant:—this was the case; and two sets of the latter were always observed.

About the time the earliest of the observations mentioned in my former paper were taken, three correspondent eclipses of the satellites of Jupiter had been observed at Canton, by

* By the late authorities, Point de Galle is placed 7° 22′ 30″ E. of Bombay. Now, allowing Bombay to be in 72° 54′ 43″, as deduced in my former paper, the longitude of Point de Galle will be 80° 17′ 13″ east.

Captain HUDDART, and at Calcutta,* which gave the difference of longitude of those places 24° 54′.

Longitude of Calcutta, by the deduction in my former Paper	88° 23′ 39″
D. longitude to Canton . . .	24 54 0
Longitude of Canton . . .	113 17 39

Captain HUDDART, by seven eclipses of the satellites, the difference in the Tables being allowed for, made the longitude of Canton 113° 19′ 7″; the mean of both is 113° 18′ 23″ east of Greenwich.

J. GOLDINGHAM.

* This information was given me at the time by Mr. J. LINDLEY, formerly Assistant at the Royal Observatory.

XXXII. *Observations on the genus Planaria.* *By* J. R. JOHNSON, *M. D. F. R. S.*

Read June 27, 1822.

I BEG leave to lay before the Royal Society a few observations on the genus PLANARIA, to which I have been more immediately led, from two or three species having been already described in the Linnæan Transactions under the genus HIRUDO.

I shall confine my remarks in the present paper to the following species: *P. cornuta, P. torva, P. brunnea, and P. lactea,* intending to give, at some future period, the farther history of this singular and extensive genus.

P. cornuta.

Planaria depressa, oblonga, nigro-cinerea, antice tentaculis instructa.

Long. 4 *lin. Lat.* 2 *lin.*

Body, blackish brown, convex above, flat beneath; fore part exsertile and retractile, with a tentaculum or feeler projecting on each side of the head; tail pointed; two ventral foramina; numerous eyes.

P. torva.

Planaria depressa, oblonga, cinerea, subtus albida, iride alba.

Long. 6 *lin. Lat.* 2 *lin.*

Body, brown on the back; grey or whitish on the belly; slightly convex above, plain beneath; truncate before, pointed behind; iris, white; two ventral foramina; eyes, two, black, in a white areola.

P. brunnea.

Planaria oblonga brunnea, linea longitudinali nigra.

Long. 5 *lin. Lat.* 2½ *lin.*

Body, dark brown, with a black dorsal longitudinal line; convex above, plain beneath; obtuse before, slightly pointed behind; two ventral foramina; numerous eyes.

P. lactea.

Planaria depressa, oblonga, alba, antice truncata.

Long. 6 *lin. Lat.* 2½ *lin.*

Body, milky-white, or cream-colour; slightly convex above, plain beneath; truncate before, sharply pointed behind; lateral margin acute, pellucid; two ventral foramina; eyes, two, deep black.

These little animals, which are delineated of their natural size, in Fig. 1, 2, 3, 4, are generally found in slow streams, assembled together in clusters, attached to the roots, or under surfaces of the leaves of aquatic plants, pieces of wood, stone, &c. In a state of rest, they are of a circular or spherical shape; when in motion, linear. They are remarkably quick and rapid in their movements, and contract and lengthen themselves with the greatest facility. From their great con-

tractility, the body consisting of a series of annular muscles, they are enabled to assume almost every variety of figure. In traversing a smooth surface, they have usually a gliding or snail-like motion, leaving a viscous tract behind ; but when the surface is rugged, they quit this gliding motion, and move forward like the leech, alternately attaching the head and tail : they do not, however, as far as I can perceive, fix the tail ; they simply draw it up, when the body is firmly retained by the strong glutinous substance with which it is invested.

The *Planariæ* are often seen traversing the surface of the water in an inverted position, and with the same facility as the *Glossoporæ*, described in a former paper. When gliding along, they move the head from side to side with rapidity, but on coming suddenly into contact with any hard substance, they immediately retract, or draw in the fore-part of the body, somewhat like the *Limax*.

On visiting the rivulet, from which I was in the habit of taking these animals for the experiments I am about to relate, I was surprised to find a large body of them *(P. torvæ)* proceeding against the current, gliding over its sandy bottom, keeping the same order as ants when passing from one of their establishments into another, and occupying a space of about twelve inches in length by two in breadth. This regular movement I observed two or three days in succession. The weather being at this time extremely temperate, had, doubtless, induced them to quit their several hiding places ; but I could not discover the purport of this proceeding.

The *Planariæ* are so gelatinous and pulpy in their texture, as scarcely to allow of any very accurate dissection. I may

however remark, that the body consists of one common cavity, with diverging lateral cells or branches, destined to contain the nutriment; being in this arrangement very analogous to the stomach of the medicinal leech.

In the absence of food, the *interanea* of these animals cannot be distinctly seen. In the *P. lactea*, they present a beautifully arborescent appearance, as delineated, highly magnified, in fig. 17. Sometimes they are of a deep purple colour, at other times of a brilliant red or dark brown, depending upon the nature of the food taken.

The ventral apertures, which are two in number, and circular, are very evident in each of the Planariæ above described, but more particularly so in the *P. torva* (fig. 6); the upper orifice gives passage to a long flexible tube, the lower one conducts to the ovarium. Of the use of this tube, which the *Planariæ* frequently project, and which nearly equals the body in length, I remained for some time ignorant, when it was discovered to me by mere accident.

Being desirous of ascertaining the proper food of these animals, as they were languishing in confinement, I threw in among them a variety of aquatic insects, worms, &c. One of them *(P. cornuta)*, after the lapse of a few minutes fastened upon a worm, immediately projecting and affixing this tube: the worm being in this way closely retained, other *Planariæ* came forward to its assistance, and thus completely overpowered it. It is astonishing with what obstinacy they maintained their hold, notwithstanding the writhings and contortions of the worm to effect its escape.

The *Planariæ* seldom attack the worm openly, seemingly aware of the difficulty of thus overcoming it, but seize upon

it, as it were, by stealth, gliding gently underneath it, and then projecting and affixing this organ, keeping a firm hold until they have concluded their repast. I have seen, at times, nearly a dozen preying upon a solitary worm. *Fig.* 16 represents the *P. cornutæ* thus engaged; at *a* and *b* this tube is seen projected. What however surprised me, was, to observe these animals, for a considerable time after, moving the head freely from side to side, still keeping the tube firmly affixed. As the worm was yet living, I imagined the sole use of this organ to be, that of effectually securing their prey. To be convinced of this, I gave them a dead worm, which they fastened upon in like manner: the head or mouth of these animals remaining, as in the former instances, unattached. This singularity could not fail of interesting me, since it appeared they received their food by this organ, and not by the mouth.

To remove any doubt which might still exist, I cut off the head of one of the *P. lacteæ*, and to the headless body presented, a day or two after, an earth-worm, upon which it soon affixed, and then inserted this tube; procuring in this way an abundant supply, sufficiently evidenced by the *interanea*, which were not before visible to the naked eye, becoming immediately conspicuous. Another *P. lactea* attached itself also to a portion of a large earth-worm, keeping its head the whole time detached, as delineated in *Fig.* 15. Here, also, the *interanea* were filled, affording an indisputable proof of nourishment having been taken in by this organ alone.

From a number of other experiments I found, that when the *Planariæ* were perfect animals, this organ was, while taking their food, in constant use; but in the event of their

being naturally or artificially divided, to which we shall soon advert, or their losing this tube, which was not unfrequently the case, they took their usual sustenance by the mouth.

I should not have entered into this detail, were I not apprehensive that I should be considered as labouring under some mistake, since it seems so contrary to the generally established law of nature, that an animal, furnished with a proper mouth, should receive its food by another organ, and that organ placed as near to the tail as to the head. Such, however, is the case; and the proofs advanced, will, I trust, be sufficiently convincing. In addition, I have to observe, that MÜLLER, after alluding to this organ, in his description of the *P. lactea,* says, "*Organum hoc nutrimento capiendo inservire vix dubitari potest.*"

Although I have repeatedly seen the young of the *P. torva* and *P. cornuta,* I have not yet been able to determine whether they are oviparous or viviparous, which I conceive to have arisen from having begun my enquiries late in the season. With respect to the *P. lactea* and *P. brunnea,* they are oviparous, producing eggs, within a membranous capsule, each capsule containing (at least those of the *P. lactea*), from 3 to 8 young.

One of the *P. lactea* deposited a capsule, July 8, which, on the 10th of August (33 days), produced 8 young.

Considering the size of the *P. lactea,* the capsule is of great magnitude, occupying nearly one-fourth of the body, forming a remarkable protuberance on the back. (fig. 5).

Six capsules were deposited by the *P. brunnea,* August 12, but of a size much less than those of the *P. lactea*; one only was productive (September 5th); it contained 2 young.

From a neighbouring pond I collected several capsules of this species, which I found attached to the under surface of the leaves of the *ranunculus aquatilis*, and found them to produce as follows:

July	26,	16 young from 5 capsules.	
August	3,	25 ———— 7 ———	
———	12,	10 ———— 3 ———	

making an average of three young to each capsule.

The young, on their immediate exclusion from the egg, are of different sizes, and remarkably active. I have sometimes seen them after taking food, so stretched, as nearly to double their previous size, a convincing proof of the great extent of the abdominal cavity.

A very singular part of the history of these animals, and what does not appear yet to have been noticed, is, that they have a double mode of perpetuating their species; 1st. by capsules, which, as we have seen, contain several young; 2ndly, by a *natural* division of the body into two portions, the head part reproducing a tail, and the tail a head, in about fourteen or more days, depending upon the state of the atmosphere.

Fig. 7 and 8 represent the *Planariæ* when about to divide to form two distinct animals. On the third day the separation of the head from the tail usually takes place, as at *fig.* 9. When undergoing this division, they remain for the most part stationary, keeping the head firmly affixed, twisting round the tail, from time to time, with a view of lessening its adhesion, and thus more readily effecting its disunion. Almost immediately after the head is liberated, it is seen to move with all the freedom of the unmutilated, perfect animal. The

tail generally remains attached, and only occasionally shifts its situation; but if touched, it moves with nearly the same quickness as the anterior extremity, preserving an uniform gliding motion.

This property, which is common to the several *Planariæ* enumerated in this paper, is most strongly marked in the *P. cornuta.*

The reproductive power of these animals, when divided, is alluded to by MÜLLER, SHAW, and others. Indeed it could scarcely be expected, after the public attention had been excited by the astonishing reproduction of the *Polype*, even when cut or divided into several minute pieces, that the reproduction of an animal, so open to common observation as the *Planaria*, should long have remained unnoticed.

The following experiments show to what a degree this reproductive power extends.

July 8. I divided several of the *Planariæ* into two equal parts: the head and tail portions of the *P. torva* were reproduced in 14 days; those of the *P. cornuta* in 17 days; of the *P. brunnea* in 15 days; and of the *P. lactea* in 19 days.

July 30. An equal number of the *P. cornutæ* and *P. lacteæ* were divided, transversely, a little below the eyes. The heads of the former were renewed in 11 days, of the latter in 14 days. The head portions were a long time in recovering the tail: in 16 days they were as delineated in *fig.* 10 and 11. As the *P. lactea* had recently taken food, the *interanea* are also exhibited.

August 9. The *Planariæ* were now divided into three equal parts: the middle portions were observed to reproduce heads and tails by the 25th (16 days). *Fig.* 12 represents, under

a magnified form, the *P. brunnea,* and *fig.* 13, the *P. cornuta,* with renewed heads and tails. The head portions show, very distinctly, a circular-like range of black dots close to the margin, which I conjecture to be the eyes; and the tail portions show the renewal of the abdominal tube.

July 30. The *Planariæ* with newly formed heads, mentioned in the first experiment, were again divided, and reproduced by August 25 (26 days). At the time of the former experiment these animals were strong and vigorous, which sufficiently accounts for the difference of time in the reproduction. *Fig.* 14 represents the head of one (*P. torva*) highly magnified, showing the eyes in the centre of the white areola.

I afterwards divided these animals into four, five, and even six parts; and what was extremely singular, each part seemed to possess the properties of a perfect animal, moving about in the water in the same gliding manner as before the separation. But when thus divided, they seldom retained their vitality beyond a few days, or if any portion did, perchance, recover its lost parts, it was a process extremely slow and tedious.

To render these experiments successful, it will be necessary to divide the *Planariæ* immediately on their being taken from their native haunts, for, when long confined, they lose their activity, and are ill calculated for the purpose. I should have observed, that, although the lost parts are generally reproduced in about a fortnight, yet a month or more is required before they take on the precise form and colour of the original animal.

The *Planariæ* with renewed organs were placed in a vessel

by themselves, and two or three, notwithstanding their previous artificial division, divided naturally, as at *fig.* 9.

Having frequently noticed several full-grown *Planariæ* with the head and tail of a greyish white, the rest of the body being brown or black, I should, without the knowledge of this re-productive property, have been led to consider them a distinct species.

In reference to the *P. lactea*, MÜLLER says; "*Unicum exemplar cujus cauda in duos lobos acutos fissa erat reperi, aliud absque oculis, et interaneis conspicuis, mere lacteum; cæterum idem.*" It seems he was not aware, or I think he would have noticed it, that the circumstance of these *Planariæ* being without eyes, &c., was occasioned by their having undergone a natural division, in order to their becoming distinct and separate animals.

J. R. JOHNSON, M.D. F.R.S. and L.S.

Bristol,
September 24, 1821.

EXPLANATION OF PLATE XLIX.

Fig. 1. *P. cornuta*, front view, natural size.
2. *P. torva*, Do. Do.
3. *P. brunnea*, Do. Do.
4. *P. lactea*, Do. Do.
5. Do. with its egg or capsule.
6. Back view of *P. torva*, showing the ventral foramina.
7. *P. cornuta*, about to undergo a natural division.
8. *P. torva*, Do. Do.

9. Head and tail of *P. cornuta,* naturally divided.
10. Head of *P. cornuta,* with tail partly renewed.
11. Head of *P. lactea,* Do. Do.
12. *P. brunnea,* with renewed head and tail.
13. *P. cornuta,* Do. Do.
14. Head of *P. torva,* highly magnified, showing the eyes in the centre of the white areola.
15. *P. lactea* attached to a portion of a large earth-worm, with the abdominal tube inserted.
16. *P. cornutæ* preying upon an aquatic worm, showing at *a* and *b* the abdominal tube.
17. *Interanea* of *P. lacteæ,* highly magnified, showing its arborescent ramifications.

XXXIII. *Some experiments and researches on the saline contents of sea-water, undertaken with a view to correct and improve its chemical analysis.* By ALEXANDER MARCET, *M. D. F. R. S. Honorary Professor of Chemistry at Geneva.*

Read June 27, 1822.

IN a paper on the temperature and saltness of various seas, which the Royal Society did me the honour to publish in their Transactions for the year 1819, I threw out a conjecture, that the sea might contain minute quantities of every substance in nature, which is soluble in water. For the ocean having communication with every part of the earth through the rivers, all of which ultimately pour their waters into it; and soluble substances, even such as are theoretically incompatible with each other, being almost in every instance capable of co-existing in solution, provided the quantities be very minute, I could see no reason why the ocean should not be a general receptacle of all bodies which can be held in solution. And although it will appear from the following account, that I have been unsuccessful in some of my attempts to prove the truth of this conjecture, it may fairly be ascribed either to a want of sufficient accuracy in our present methods of chemical analysis, or of the requisite degree of skill in the operator.

Some time after the communication to which I have just referred, an extraordinary statement was pointed out to me, upon the authority of ROUELLE, a French chemist of the last century, from which it appeared that mercury was contained

in sea salt:* and I saw soon after in the '*Annales du Musée*,' Vol. VII. a paper by the celebrated chemist Proust, who, in a great measure, confirmed that statement, by announcing that he had found traces of mercury in all the specimens of marine acid which he had examined.

Improbable as the fact appeared, I thought it worth while to repeat the experiment, and to take that opportunity of making some collateral researches upon other substances, much more likely than mercury to be discovered in sea water.

For this purpose I availed myself of the kindness of my friend Mr. John Barry,† who happened to be in the vicinity of Portsmouth, to supply me with specimens of sea-water, carefully concentrated upon the spot, in vessels of Wedgwood ware, and with scrupulous attention to cleanliness in the process. Accordingly he was so obliging, as not only to send me a quantity of brine evaporated under his own eye, in the manner just mentioned, but he also collected for me a valuable series of specimens from the salt works near Portsmouth, from all the stages of the process, so as to afford me an opportunity of investigating with accuracy, all the chemical circumstances of this interesting branch of national œconomy. Finding myself, however, much pressed by time at this late period of the session, I shall, after briefly adverting to Rouelle's supposed discovery, confine myself in this communication to a few observations which I have made on sea-water itself; keeping out of view, for the present, the topic

* See 'Journal de Médecine,' Vol. xlviii. 1777, page 322.

† Mr. John Barry, of Plough Court, inventor of a new and valuable process for preparing extracts in vacuo, &c.

of salt-making, which, however, I intend to resume at some future period, in a more complete and satisfactory manner.

I first attempted to detect mercury in a specimen of *bay-salt*, such as is obtained in the salt-works near Portsmouth, by spontaneous evaporation. This variety of salt forms large crystals, but is always more or less contaminated by earthy matter, which gives it a dirty appearance. It has, probably, a general resemblance to the French *Sel de Gabelle*, which is more impure still, though, I believe, obtained in a similar manner.*

Eight ounces of this salt were put into a coated retort connected with a receiver, and about four ounces of nitrous acid were poured upon it. A pretty brisk action took place, which was farther increased by the application of heat; fumes of chlorine were immediately disengaged, and a reddish fluid condensed in the receiver; the heat was continued, and gradually raised in a charcoal fire till no acid or moisture any longer came over; at which time a new emission of red fumes indicated that the nitrate formed in the retort was beginning to part with its acid: minute drops of fused salt soon bedewed the upper part and neck of the retort, so as to be mistaken, at first, for a sublimate. This, however, proved to be almost solely muriate of soda; and on careful examination, it did not appear to contain the smallest atom of corrosive sublimate.

I next dissolved five or six pounds of bay-salt in water, and collected in a filter the insoluble earthy sediment, in

* The name of *bay-salt* is often applied to foreign, as well as British salt, and in general it simply denotes that the salt has been obtained by spontaneous evaporation.

which ROUELLE stated that the quick-silver was usually found. This sediment being carefully dried, and heated to redness in a coated retort, a white sublimate arose, and condensed on the neck of the retort; but this sublimate proved to be muriate of ammonia, and did not contain the smallest portion of corrosive sublimate or other mercurial salt. This sal-ammoniac, though evidently formed during the distillation from the vegetable and animal matter contained in the sediment, suggested to me the idea of looking for ammonia amongst the contents of sea-water.

I now submitted some *Sel de Gabelle*, which I had procured from Calais for the purpose, to similar experiments, and the sediment, also, was carefully examined. The result was essentially the same as with the bay-salt. After adding nitric acid to the salt, the heat was gradually pushed to redness; and when all the moisture was evaporated, a white sublimate appeared, as in the former case, which, in this instance, proved to consist almost entirely of nitrate of soda; but always without the least particle of mercurial salt, and without any muriate of ammonia.*

I therefore think myself justified in concluding that the mercury, which other chemists have detected in sea-salt or its products, must have been introduced there from some local or accidental circumstances.

In experiments upon sea-salt, or in general upon the saline contents of the sea, it is obvious that, in order to exclude sources of error, it is necessary to operate upon pure sea-

* In the former experiment the sublimate was principally muriate of soda, owing, no doubt, to the decomposition having been less complete, and the operation less gradually conducted than in the latter experiment.

water, and not upon salts obtained from it by the usual processes in the large way, these being always more or less contaminated by the clay pits in which the evaporation is carried on, by the metallic boilers, or other adventitious causes. I therefore now turned my attention to the sea-water itself, and in particular the perfectly pure and transparent specimen of concentrated brine from the Channel, which I have above mentioned. Mr. BARRY procured this water near Bembridge floating light, about two miles N. E. of the eastern extremity of the Isle of Wight, and the evaporation which it had undergone at Portsmouth, had only separated from it a quantity of calcareous matter, principally selenite.*

A few pounds of this water were evaporated nearly to dryness, at a gentle heat, so as to reduce the mother liquor to the smallest possible quantity. This liquor was suffered to drain off, and reserved for experiments, as it is in this fluid that any new ingredients are most likely to be detected.

I had suspected that some nitric salt might be found in sea-water, but in this I was disappointed. The discrimination by the shape of the crystals being in this instance scarcely practicable, the mode which I employed for detecting it, consisted in concentrating the bittern in a glass tube or retort, till it began to deposit solid matter, then adding sulphuric acid and gold-leaf, and boiling the mixture; the gold-leaf was not in

* The water, immediately on being raised from the sea, had been allowed to stand a sufficient time to deposit the earthy particles suspended in it, by which means it had become beautifully transparent. 100 pounds of the water produced only 3 grains of earthy sediment, in which I could discover nothing but carbonate of lime and oxide of iron. It is in this sediment, according to ROUELLE, that mercury is to be found. I need hardly say that I could not detect in it the least particle of that metal.

the least acted upon, nor was any smell of nitric acid perceived; but on adding the smallest quantity of nitre to the same mixture, the gold was dissolved, and the smell of aqua regia was instantly perceived.*

A portion of the said bittern was next examined by appropriate re-agents, with a view to detect any minute quantity of earths or metals, especially alumina, silica, iron and copper, which former inquirers might have overlooked; but I could find no other earth except magnesia: and to my surprise, I did not find in the bittern the least particle of lime; which proves that sea-water contains no muriate of lime, as had been generally supposed. I was equally unsuccessful in my attempts to detect iron or copper, by the most delicate tests. In fact, neither alkalies, nor alkaline carbonates, precipitated any other substance from the bittern of sea-water, except magnesia.

The deposit obtained at Portsmouth during the early period of the concentration of the water, being analyzed, I found it to consist of selenite, mixed with a little muriate of soda, and a portion of carbonate of lime. The presence of this last subtance in sea-water, in a state of perfect solution, being, I believe, a new fact, I neglected no means of establishing it with certainty, an object which was accomplished without difficulty.†

Carbonate of magnesia having been supposed by some

* For this easy and elegant process for detecting nitric acid, a point attended with difficulty, I am indebted to Dr. WOLLASTON.

† The deposit was treated with acetic acid, which occasioned an effervescence. The clear liquor being then decanted off, and evaporated to dryness, and alcohol added, acetate of lime was found in the filtered alcoholic liquor.

chemists to exist in sea-water, I looked for it in the same deposit; but I could not detect the least portion of it by the most delicate tests.*

I next turned my attention to the alkaline salts of sea-water; and here I was more fortunate; as I succeeded in ascertaining beyond a doubt, that sea-water contains ammonia, as it yielded sal ammoniac by evaporation and sublimation. This result was easily obtained. Some of the bittern being evaporated to dryness in a retort, and a low red heat applied, a white sublimate appeared in the neck of the retort, which proved to be muriate of ammonia. The mode in which this substance was identified was as follows:

1. The sublimate was re-dissolved in water, re-evaporated to dryness, and again sublimed by the heat of a spirit lamp.

2. This new sublimate being again dissolved, and solution of magnesia and phosphoric acid added, a triple phosphate was formed.

3. On adding caustic potash to the solution, and bringing the mouth of a phial containing muriatic acid close to the vessel, abundant white fumes appeared.

4. The sublimate gave precipitates both with muriate of platina and nitrate of silver.†

Sulphate of soda having been mentioned by many chemists, as one of the constituents of sea-water, I endeavoured to ascertain, whether or not it existed in it. But all attempts

* Namely, solution of the mass in dilute muriatic acid; precipitation of the lime, and addition of phosphate of ammonia to the filtered liquor.

† As it did not enter into my plan, on this occasion, to turn my attention to the estimation of proportions or precise quantities, I have not attempted to estimate exactly the proportion which ammonia bears to the other ingredients of sea-water; but as its presence can easily be shown in 100 grains of the bitter salts, its quantity cannot be extremely minute.

to detect this salt in the bittern by crystallization were fruitless, though great pains were taken for that purpose; and I feel the more confident that there is no sulphate of soda in sea-water, as the presence of this salt, in any but the most minute quantities, would be quite incompatible with our knowledge of chemical affinities. For since there are, co-existing in sea-water, muriate of soda and sulphate of magnesia, it is evident that sulphate of soda would decompose muriate of magnesia, which salt is known to be in sea-water. And again we know, that sea-water contains sulphate of lime and muriate of soda; therefore it cannot contain sulphate of soda; for in that case we should have muriate of lime, which I have shown to be contrary to fact.

The last circumstance which I shall at present notice, relates to the state in which potash exists in sea-water.* Potash is found, by its appropriate re-agents, principally in the bittern; but it is found also among the salts which are separated from it, especially in the latter period of crystallization. By farther and repeated evaporation of the bittern, and successive separation of the mother water remaining after the removal of the crystals formed, various distinct crystals were obtained possessing their characteristic form, namely, prismatic sulphate of magnesia, cubic and star-shaped muriate of soda, and rhombic crystals, quite different from either of the other salts. These crystals, which were evidently portions of an oblique rhombic prism, being carefully separated and washed with water and alcohol, proved to be a triple salt of sulphate of

* It will be recollected, that the presence of potash in sea-water, though announced by myself in the paper on sea-water to which I have before alluded, was Dr. WOLLASTON's discovery. I have likewise to mention, that the above experiments respecting the state in which it exists, were either made by him or at his suggestion.

potash and magnesia; a salt so easily analyzed, that it would be quite superfluous to relate the particulars of the process.

It now remained to be ascertained, whether potash might not also exist in sea-water in the state of muriate of potash, or of triple muriate of potash and magnesia. That a considerable quantity of potash remains in the bittern, even after the separation of the triple sulphate, is easily ascertained; and by careful evaporation it may be made to crystallize as a triple salt in rhombic crystals; but the constitution of this salt is so delicate, that it is liable to be separated into muriate of potash and muriate of magnesia by water alone; and it is with certainty decomposed by alcohol, which takes up the magnesian muriate, and leaves the other undissolved.

From the foregoing observations and experiments it may, therefore, be inferred,

1st. That there is no mercury, or mercurial salt, in the waters of the ocean.

2dly. That sea-water contains no nitrates.

3dly. That it contains sal ammoniac.

4thly. That it holds carbonate of lime in solution.

5thly. That it contains no muriate of lime.

6thly. That it contains a triple sulphate of magnesia and potash.

Some of these circumstances will, of course, require that former analyses of sea-water, and my own in particular, should be corrected and revised; but this I shall not attempt to do, until I have obtained farther, and still more precise information on the subject.

Harley Street, 20 June, 1822.

XXXIV. *On the ultimate analysis of vegetable and animal substances.* *By* Andrew Ure, *M. D. F. R. S.*

Read June 27, 1822.

The improvements lately introduced into the analysis of vegetable and animal compounds, with the investigation of the equivalent ratios, in which their constituent elements, carbon, hydrogen, oxygen, and azote are associated, have thrown an unexpected light into this formerly obscure province of chemical science. While the substitution by M. Gay Lussac, of black oxide of copper for the chlorate of potash, has given peculiar facility and elegance to *animal* analysis, it may be doubted whether, in those cases, where the main object of inquiry is the proportion of carbon, it has not, frequently, led to fallacious results. As the quantity of this element is inferred from the volume of carbonic acid evolved in the decomposition of the organic matters, such of their particles as happen not to be in immediate contact with the cupreous oxide, will remain unconverted into carbonic acid; and thus the proportion of carbon will come to be underrated; an accident which cannot occur with chlorate of potash, since the carbonaceous matter is here plunged in an ignited atmosphere of oxygen. It is probably to this cause, that we must refer the discrepant results in the analysis of pure sugar, between M. M. Gay Lussac, Thenard, and Berzelius, on the one hand, and Dr. Prout, on the other; the former

gentlemen assigning about 43 parts in the hundred of carbon, while the latter states the carbon at only 40.*

The objects of the present paper are, first to indicate, and endeavour to remove several sources of fallacy attending the method with peroxide of copper; and next, to exhibit the results of its application to a considerable series of vegetable and animal compounds.

Peroxide of copper, prepared by igniting the pure nitrate of this metal, is, like yellow oxide of lead, and many other metallic oxides, readily absorbent of a small portion of humidity from the air, the quantity of which depends, in some measure, on the length of time during which it has suffered ignition. If exposed to a red heat, merely till the vapours of nitric acid are expelled, 100 grains of the oxide will absorb, in the ordinary state of the atmosphere, from one-tenth to two-tenths of a grain of moisture in the space of an hour or two; and about one half of the above quantity in a very few minutes. The French chemists, who have operated most with this agent, seem to be well aware of this circumstance, for they direct the peroxide to be used immediately after ignition, and to be triturated with the organic matter in a hot mortar of agate or glass. Yet this precaution will not entirely prevent the fallacy arising from the hygrometric action; for I find that peroxide thus treated does absorb, during the long trituration essential to the process, a certain quantity of moisture, which, if not taken into account, will produce serious errors in the analytical results. It is better therefore to leave the powdered peroxide intended for research, exposed for such time to the air, as to bring it to hygrometric repose,

* Dr. PROUT has informed me, since the above paper was written, that he re-triturated and re-ignited the contents of his tubes, in the analysis of sugar.

then to put it up in a phial, and by igniting one hundred grains of it in a proper glass tube, sealed at one end, and loosely closed with a glass plug at the other, to determine the proportion of moisture which it contains. This, then, indicates the constant quantity to be deducted from the loss of weight which the peroxide suffers in the course of the experiment. The mortar should be perfectly dry, but not warm.

Experimenters have been at great pains to bring the various organic objects of research to a state of thorough desiccation before mixing them with the peroxide of copper; but this practice introduces a similar fallacy to that above described. We ought, therefore, after having made them as dry as possible by the joint agencies of heat, and an absorbent surface of sulphuric acid in vacuo, to expose them to the air till they also come into hygrometric repose, noting the quantity of moisture which they imbibe, that it may be afterwards allowed for. The plan which I adopt for the purpose of desiccation seems to answer very well. Having put the pulverulent animal or vegetable matter into short phials, furnished with ground glass stoppers, I place the open phials in a large quantity of sand heated to 212° F. in a porcelain capsule, and set this over a surface of sulphuric acid in an exhausted receiver. After an hour or more the receiver is removed, and the phials instantly stopped. The loss of weight shows the total moisture which each of them has parted with; while the subsequent increase of their weight, after leaving them unstopped for some time in the open air, indicates the amount of hygrometric absorption. This is consequently the quantity to be deducted in calculating experimental results.

Many chemists, particularly in this country, have employed the heat of a spirit lamp, instead of that produced by the combustion of charcoal, for igniting the tube in which the mixed materials are placed. I have compared very carefully both methods of heating, and find that for many bodies, such as coal, and resin, which abound in carbon, the flame of the lamp is insufficient; while its application being confined at once to a small portion of the tube, that uniform ignition of the whole, desirable towards the close of the experiment, cannot be obtained. I was hence led to contrive a peculiar form of furnace, in which, with a handful of charcoal, reduced to bits about the size of small filberts, an experiment may be completed, without anxiety or trouble, in the space of half an hour. Since I have operated with this instrument, the results on the same body have been much more consistent, than those previously obtained with the lamp; and it is so convenient, that I have sometimes finished eight experiments in a day.

Fig. 1. (Plate L.) represents the whole apparatus, as when in action. Fig. 2 is a horizontal section of the furnace, in which we perceive a semi-cylinder of thin sheet iron, about 8 inches long and $3\frac{1}{2}$ wide, perforated with holes, and resting on the edge of a hollow prism of tin-plate, represented more distinctly in fig. 3, where *n* shows a slit, through which the sealed end of the glass tube may be made to project, on occasion. *i*, is a handle attached to the semi-cylinder, by which it may be slid backwards or forwards, and removed at the end of the process. *d*, is a sheath of platinum foil, which serves, by aid of a wire laid across, to support the middle of the tube, when it is softened by ignition. At *g*, the plates

which close the ends of the semi-cylinder and tin-plate prism, rise up a few inches to screen the pneumatic apparatus from the heat. A third occasional screen of tin-plate is hung on at *f*. All these are furnished with slits for the passage of the glass tube. This is made of crown glass, and is generally about 9 or 10 inches long, and $\frac{3}{10}$ of internal diameter. It is connected with the mercurial cistern by a narrow tube and caoutchouc collar. This tube has a syphon form, and rises about an inch within the graduated receiver at *e*. By this arrangement, should the collar be not absolutely air-tight, the pressure of the column of mercury causes the atmospheric air to enter at the crevice, and bubbles of it will be seen rising up without the application of heat. At the end of the operation, the point of the tube *e*, is always left above the surface of the mercury, the quantity of organic matter employed being such as to produce from 6 to 7 cubic inches of gaseous product, the volume of the graduated receiver being 7 cubic inches.

As the tubes with which I operate have all the same capacity, viz. half a cubic inch; and as the bulk of materials is the same in all the experiments, one experiment on the analysis of sugar or resin, gives the volume of atmospheric air due to the apparatus, which volume is a constant quantity in the same circumstances of ignition. And since the whole apparatus is always allowed to cool to the atmospheric temperature, the volume of residual gas in the tubes comes to be exactly known, being equal, very nearly, to the primitive volume of atmospheric air left after the absorption of the carbonic acid in the sugar or resin experiment.* Thus this

* If a be the capacity of the graduated receiver, and b the spare capacity of the tubes, then the above volume is $b - \frac{b}{a+b}$.

quantity, hitherto ill appreciated or neglected in many experiments, though it is of very great consequence, may be accurately found. At *k*, fig. 2, a little tin-plate screen is shown. It is perforated for the passage of the tube, and may be slid along, and left at any part of the semi-cylindric cage, so as to preserve from the influence of the heat, any requisite portion of the sealed end of the tube. At fig. 4 is seen the shape of the little bulb, into which I introduce the proper weight of ether, alcohol, naphtha, or other volatile liquids, which are destined for analysis. After weighing it exactly, it is immediately slid down to the bottom of the tube, and covered with 150 or 200 grains of peroxide of copper. The bulb has a capacity equal to 3 grain measures of water, and its capillary point is sometimes closed with an inappreciably small quantity of bees wax, to prevent the exhalation of the liquid, till the peroxide be ignited.

b is a cover to the furnace, with an oblong orifice at its top. It serves for a chimney, and may be applied or removed by means of its handle, according as we wish to increase or diminish the heat. *c c* are tin cases inclosing corks, through which the iron wires are passed, that support the whole furnace at any convenient height and angle of inclination.

The tightness of the apparatus at the end of the process, is proved by the rising of the mercury in the graduated receiver, by about one-tenth of an inch, as the tube becomes refrigerated.

My mode of operating with the peroxide of copper is the following:

I triturate very carefully in a dry glass mortar, from 1 to $2\frac{1}{2}$ grains of the matter to be analyzed, with from 100 to 140

grains of the oxide. I then transfer it, by means of a platinum-foil tray and small glass funnel, into the glass tube, clearing out the mortar with a metallic brush. Over that mixture I put 20 or 30 grains of the peroxide itself, and next, 50 or 60 grains of clean copper filings. The remaining part of the tube is loosely closed with 10 or 12 grains of amianthus, by whose capillary attraction the moisture evolved in the experiment is rapidly withdrawn from the hot part of the tube, and the risk of its fracture thus completely obviated. The amianthus serves moreover as a plug, to prevent the projection of any minute particles of filings, or of oxide, when the filings are not present. The tube is now weighed in a very delicate balance, and its weight is written down. A little cork, channelled at its side, is next put into the tube, to prevent the chance of mercury being forced backwards into it, by any accidental cooling or condensation. The collar of caoutchouc is finally tied on, and the tube is placed, as is shown in fig. 2, but without the plate *k*, which is employed merely in the case of analyzing volatile liquids. A few fragments of ignited charcoal are now placed under the tube at the end of the furnace next to the cistern, and the remaining space in the semi-cylinder is filled up with bits of cold charcoal. The top, *b*, may then be put in its place, when the operation will proceed spontaneously, the progressive advance of the ignition from one end to the other being proportioned to the expansion of glass, so that the tube very seldom cracks in the process. Indeed I have often used the same tube for a dozen experiments, in the course of which it became converted into *vitrite*, or Reaumur's porcelain.

Since the evolved gas is saturated with moisture, I reduce

a great number, seem to consist of

Carbon	0.8345
Hydrogen	0.1318
Water	0.4337
	1.4000

And in 1 grain we shall have

Carbon	0.5960	3 atoms	2.25	60.00
Hydrogen	0.1830	4 atoms	0.50	13.33
Oxygen	0.2710	1 atom	1.00	26.66
	1.0000		3.75	100.0

Or, 3 volumes olefiant gas $= 3 \times 0.9722 = 2.9166$
2 . . vapour of water . $2 \times 0.625 = 1.25$

4.1666

which suffering a condensation equal to the whole vapour of water, will give an ethereous vapour, whose specific gravity is 2.5.

The proportion of the constituents of sulphuric ether, deduced by M. GAY LUSSAC from the experiments of M. TH. DE SAUSSURE, are 2 volumes olefiant gas + 1 volume vapour of water, which 3 volumes are condensed into 1 of vapour of ether, having a specific gravity = 2.58. The ether which I used had been first distilled off dry carbonate of potash, and then digested on dry muriate of lime, from which it was simply decanted, according to the injunction of M. DE SAUSSURE. Whether my ether contained more aqueous matter than that employed by the Genevese philosopher, or whether the difference of result is to be ascribed to the difference in the mode of analysis, must be decided by future researches.

By analogous modes of reduction, the following results were deduced from my experiments. I ought here to state, that in many cases the materials, after being ignited in the tube, and then cooled, were again triturated in the mortar, and subjected to a second ignition. Thus, none of the carbon could escape conversion into carbonic acid. I was seldom content with one experiment on a body; frequently six or eight were made.

Table of organic analyses.

	Substance.	Carbon.	Hydrogen.	Oxygen.	Azote.	Water.	Excess.
1	Sugar - - - -	43.38	6.29	50.33		56.62	
							Oxygen
2	Sugar of diabetes - -	39.52	5.57	54.91		51.13	10.35
3	Starch - - -	38.55	6.13	55.32		55.16	6. 3
4	Gum Arabic - - -	35.13	6.08	55.79	3 ?	54.72	7.15
							Hydrogen
5	Resin - - - -	73.60	12.90	13.50		15.20	11.20
6	Copal - - -	79.87	9.00	11.10		12. 5	7. 6
7	Shell lac - - -	64.67	8.22	27.11		30.51	4.82
8	Resin of guaiac. - -	67.88	7.05	25.07		28.	3.93
9	Amber - - -	70.68	11.62	17.77		20. 0	9.40
10	Yellow wax - -	80.69	11.37	7.94		8.93	10.39
11	Caoutchouc - - -	90.00	9.11	0.88		.99	9.00
12	Splent coal - - -	70.90	4.30	24.80		27.90	1.20
13	Cannel coal - - -	72.22	3.93	21.05	2. 8	23.68	1.30
14	Indigo - - -	71.37	4.38	14.25	10.	16. 0	2.52
15	Camphor - - -	77.38	11.14	11.48		12.91	9.71
16	Naphthaline - - -	91. 6	7.`7	0.70?		0.79?	
17	Spermaceti oil - -	78.91	10.97	10.12		11.34	9.71
18	Common oil of turpentine	82.51	9.62	7.87		8.85	8.64
19	Purified oil of turpentine	84. 9	11. 5	3. 6		4. 0	11. 1
20	Naphtha - - -	83.04	12.31	4.65		5.23	11.73
21	Asiatic castor oil - -	74.00	10.29	15.71		17.67	8.33
22	Alcohol, sp. gr. 0.812 -	47.85	12.24	39.91		44. 9	7.25
23	Ether, specific gravity 0.70	59.60	13. 3	27. 1		30. 5	9. 9
							Oxygen
24	Bleached Silk - -	50.69	3.94	34.04	11.33	35.43	2.55
25	Cotton - - -	42.11	5.06	52.83		45.56	12.33
26	Flax, by Lee's process	42.81	5.·5	51. 7		49. 5	7. 7
27	Common flax - -	40.74	5.57	52.79	0. 9	50.16	8. 2
28	Wool - - -	53. 7	2.80	31. 2	12. 3	25. 7	8. 3
							Hydrogen
29	Cochineal - - -	50.75	5.81	36.53	6.91	39. 6	14. 1
30	Cantharides - - -	48.64	5.99	36.29	9.08	40.83	14.53
31	Urea - - -	18.57	5.93	43.68	31.82	49.14	0.47
32	Benzoic acid - -	66.74	4.94	28.32		31.86	1.˙4
							Oxygen
33	Citric acid - - -	33.00	4.63	62.37		41.67	25.33
34	Tartaric acid - -	31.42	2.76	65.82		24.84	43.74
35	Oxalic acid - - -	19.13	4.76	76.20		42.87	38.09
36	Ferroprussic acid - -	36.82	27.89 of iron.		35.29		

Remarks on the preceding analyses.

1. Sugar. The sugar which I employed had been purified by Mr. HOWARD's steam process, and was so well stove-dried, that it lost no appreciable portion of its weight, when enclosed along with sulphuric acid in *vacuo*. The diabetic sugar has a manifest excess of oxygen, which I believe to be the case with all weak sugars, as they are called by the sugar refiners. I consider this excess of oxygen as the chief cause which counteracts crystallization, and therefore the great obstacle to the manufacturer. The smallest proportion of carbon, which I have ever found in any cane sugar, was upwards of 41 per cent. The experiments on starch and gum were among the earliest which I made, and the results differ so much from those given by other experimenters, that I shall repeat the analyses at the earliest opportunity. The constituents of the above three bodies, referred to the prime equivalent scale, will be approximately as follows:

Sugar.			Starch.			Gum.		
Carbon 5 atoms	3.75	45.4	5 atoms	3.75	40.5	4 atoms	3.0	35.25
Oxygen 4 ——	4.00	48.5	5 ——	5.00	54.0	5 ——	5.0	58.90
Hydrogen 4 ——	0.50	6.1	4 ——	0.50	5.5	4 ——	0.5	5.85
	8.25	100.0		9.25	100.0		8.5	100.00

I conceive the purest and strongest sugar to be constituted as here represented.

All the elementary principles of organic nature may be considered as deriving the peculiar delicacy of their chemical equilibrium, and the consequent facility with which it may be subverted and new modelled, to the multitude of atoms

grouped together in a compound. On this view, none of them should be expected to consist of a single atom of each component. The allurement of theoretic simplicity has led some ingenious philosophers to represent sugar by 1 atom of carbon, 1 atom of oxygen, and 1 atom of hydrogen; or of 40 carbon, $53\frac{1}{3}$ oxygen, and $6\frac{2}{3}$ hydrogen in 100 parts. But I am satisfied that all sound specimens of sugar will yield considerably more carbon than 40 *per cent.*

Starch is liable to a similar deterioration with sugar; that is, some species of it make a much firmer coagulum with hot water than others; a difference probably due to the proportion of oxygen. The starch here employed was that of commerce, and was not chemically desiccated: hence, the redundancy of water beyond the equivalent proportion. A little hygrometric moisture was present also in the gum, as it was not artificially dried. A note of interrogation is placed after azote. That doubt will I trust be solved, when I complete my analyses of grains, roots, and leaves, with the view of tracing the origin of azote in the bodies of graminivorous animals.

We now come to a class of bodies in which the hydrogen predominates over the oxygen. With regard to resin, I believe the quantity of its carbon to be somewhat underrated in the table. Though three experiments were made on it, I now perceive that I had omitted to re-triturate and re-ignite; and the carbon of resin is very difficult of oxygenation. Its true composition is probably,

Carbon	8 atoms	6.0	75.00
Hydrogen	8 ——	1.0	12.50
Oxygen	1 ——	1.0	12.50
		8.0	100.00

A still more symmetric arrangement would be derived from

Carbon	8 atoms	6.0	73.9
Hydrogen	9 ——	1.125	13.8
Oxygen	1 ——	1.000	12.3
		8.125	100.0

This proportion corresponds to 8 atoms of olefiant gas and 1 atom of water; and I think it is very possibly the true constitution of resin. Had the loss of weight suffered by the contents of the tube, during their ignition, been a few hundredth parts of a grain more, the experimental result would have coincided with this theoretical view. Copal approaches to

Carbon	10 atoms	7.5	80.30
Hydrogen	7 ——	0.875	9.36
Oxygen	1 ——	1.000	10.34
		9.375	100.00

Lac may be nearly represented by

Carbon	6 atoms	4.5	64.3
Hydrogen	4 ——	0.5	28.5
Oxygen	2 ——	2.0	7.2
		7.0	100.0

or 2 atoms of olefiant gas + 1 atom carbonic oxide; that is equal weights of these two binary compounds; for

2 atoms of olefiant gas $= 2 \times (0.75 + 0.125) = 1.75$
1 atom carbonic oxide . . . $= 1.75$

Referred to volumes, we shall have lac to consist of equal parts of the two gases.

Resin of guaiac. gives

Carbon	7 atoms	5.25	67.7
Hydrogen	4 ——	0.50	6.5
Oxygen	2 ——	2.00	25.8
		7.75	100.0

Although the experiments on amber were conducted carefully with re-trituration and re-ignition, no good atomic configuration of it has occurred to me. It approaches to 10 carbon + 10 hydrogen 10 hydrogen + 2 oxygen.

Wax is apparently composed of

Carbon	13 atoms	9.75	80.4
Hydrogen	11 —	1.375	11.3
Oxygen	1 —	1.000	8.3
		12.125	100.0

or in other words, of 11 atoms olefiant gas + 1 atom carbonic oxide + 1 atom carbon. Had the experiment given a very little more hydrogen, we should have had wax as consisting of 12 atoms olefiant gas + 1 atom carbonic oxide. This is possibly the true constitution.

Caoutchouc seems to consist of

Carbon	3 atoms	2.25	90
Hydrogen	2 —	0.25	10
		2.50	100

Or it is a sesqui-carburetted hydrogen. The oxygen deduced from experiment is in such small quantity, as to leave a doubt whether it be essential to this body, or imbibed in minute quantity from the air during its consolidation.

Splent or slate coal, specific gravity 1.266, abstracting its incombustible ashes, approaches in constitution, to

Carbon	7 atoms	5.25	70.00
Hydrogen	3 —	0.375	3.40
Oxygen	2 —	2.000	26.6
		7.625	100.0

Cannel coal from Woodhall, near Glasgow, specific gravity 1.228, resembles a compound of

Carbon	9 atoms	6.750	73.9
Hydrogen	3 ——	0.375	4.2
Oxygen	2 ——	2.000	21.9
		9.125	100.0

In both of these bodies there is an excess of carbon beyond the 3 atoms of olefiant gas and 2 of carbonic oxide. The former coal has 2 extra atoms of carbon, and the latter 4 atoms. Hence, this coal is found at the Glasgow gas works to yield a very rich burning gas. I do not know whether the azote be essential to the constitution of this coal, or accidentally introduced from animal remains at the formation of the strata.

The elements of indigo may be grouped as follows:

Carbon	16 atoms	12.0	72.70
Hydrogen	6 ——	0.75	4.55
Oxygen	2 ——	2.00	12.15
Azote	1 ——	1.75	10.60
		16.50	100.00

or, in other terms, we shall have

1 atom cyanogen, 6 atoms olefiant gas, 2 atoms carbonic oxide, and 6 atoms of carbon in excess.

I had intended to pursue, at considerable detail, my researches on this curious azotized product of vegetation, but the subject having been lately taken up, and ingeniously prosecuted by my pupil and friend, Mr. WALTER CRUM, I was induced to leave it in his hands. He announced to me the presence of hydrogen in indigo, before I had analyzed this substance myself; and drew my attention, particularly, to

the fallacy occasioned by the hygrometric water of the peroxide of copper. It is likely that some slight modification may require to be made in my tabular proportion of the constituents, for I did not resume the subject of indigo, after I had become most familiar with the manipulations.

Camphor is very nearly represented by

Carbon	10 atoms	7.5	78.02
Hydrogen	9 ——	1.125	11.58
Oxygen	1 ——	1.0	10.40
		9.625	100.00

or 9 atoms olefiant gas + 1 atom carbonic oxide. Naphthaline is, in my opinion, a solid bi-carburet of hydrogen, consisting of

Carbon	2 atoms	1.5	92.9
Hydrogen	1 ——	0.125	7.1
		1.625	100.0

It is very difficult, even by the best regulated ignition, to resolve the whole carbon of this very volatile body into carbonic acid; hence, the carbon may come to be underrated in the result. Naphthaline is obtained during the rectification of the petroleum of the coal gas works. It is found encrusting the pipes in the form of a greyish crystalline mass; and when purified by a second sublimation at the temperature of about 220°, it forms beautiful thin plates, white and glistening. It has a powerful petroleum odour. With brine of the specific gravity 1.048, these plates, when once thoroughly wetted (which is difficult to effect) remain in equilibrium; that is, float in any part of the liquid. That number, therefore, represents the specific gravity of naphthaline. It is insoluble in water, but very soluble in ether, and moderately so in alcohol. With iodine, it fuses at a gentle

heat into a brown liquid, forming as it cools a solid resembling plumbago, which dissolves readily in alcohol, and is thrown down by water. Naphthaline is soluble in oils. In water heated to 168° F. it fuses, and remains like oil at the bottom of the liquid; but when stirred it rises, and spreads on the top in little oily patches. At 180° it rises spontaneously from the bottom in oily globules, which, as the temperature is raised, dissipate in the air, undergoing motions similar to those of camphor floating on water.

Spermaceti oil is constituted apparently of

Carbon	10 atoms	7.5	78.0
Hydrogen	9 ——	1.125	11.8
Oxygen	1 ——	1.0	10.2
		9.625	100.0

or, in other words, of 9 atoms olefiant gas + 1 atom carbonic oxide. The experimental proportion is, however, more nearly,

Carbon	10 atoms	7.5	79.0
Hydrogen	8 ——	1.0	10.5
Oxygen	1 ——	1.0	10.5
		9.5	100.0

There is here an atom of carbon in excess.*

Common oil of turpentine, specific gravity 0.888, comes very closely to the following arrangement:

Carbon	14 atoms	10.5	82.35
Hydrogen	10 ——	1.25	9.80
Oxygen	1 ——	1.00	7.85
		12.75	100.00

* This is probably the truer view. The former would make it coincide with camphor.

Oil of turpentine, purified with alcohol by Dr. NIMMO's method, seems to approach to the constitution of naphtha, or of a mere carburet of hydrogen. Its specific gravity is 0.878. But as from the mode of preparing it, a minute portion of alcohol may remain in it, I do not think it necessary to investigate its atomical structure.

Naphtha, specific gravity 0.857, obtained by distillation from petroleum, is very nearly represented by,

Carbon	22 atoms	16.50	82.5
Hydrogen	20 ——	2.50	12.5
Oxygen	1 ——	1.00	5.0
		20.00	100.0

It therefore consists of 20 atoms olefiant gas, 1 atom carbonic oxide, and 1 atom of carbon held in solution.

Castor oil is an interesting unctuous body, from its great solubility in alcohol. It consists nearly of

Carbon	7 atoms	5.25	75.00
Hydrogen	6 ——	0.75	10.70
Oxygen	1 ——	1.00	14.30
		7.00	100.00

It is composed therefore of 6 atoms olefiant gas + 1 atom carbonic oxide; or in volumes of 3 olefiant gas + 1 carbonic oxide.

Alcohol, specific gravity 0.812, is composed very nearly of

Carbon	3 atoms	2.250	46.15
Hydrogen	5 ——	0.625	12.82
Oxygen	2 ——	2.000	40.03
		4.875	100.00

or, of 3 atoms olefiant gas = 2.625.

2 —— water . = 2.25.

Cochineal seems to be made up of

Carbon	15 atoms	-	11.250	-		50.20
Hydrogen	11 ——	-	1.375	-	-	6.15
Oxygen	8 ——	-	8.000	-	-	35.85
Azote -	1 ——	-	1.750	-	-	7.80
			22.375			100.00

Cantharides approximate to

Carbon	11 atoms	- -	9.75	-	-	49.4
Hydrogen	10 ——	- -	1.25	-	-	6.3
Oxygen	7 ——	- -	7.00	-	-	35.4
Azote -	1 ——	- -	1.75	-	-	8.9
			19.75			100.0

My result with urea differs so considerably in the proportion of azote, from that of Dr. Prout and M. Berard, that I am disposed to doubt of the accuracy of my experiments, though they were made with the utmost care, and were most consistent in the repetition. I could perceive no smell whatever of nitrous gas in the gaseous products, which were made to traverse a column of copper filings 3 inches long, in a state of ignition. I shall renew the inquiry on urea, and employ the lowest temperature compatible with the formation of carbonic acid.

The prime equivalent of benzoic acid crystals, I find by saturation with water of ammonia, to be 14.5; and it consists apparently of

Carbon	13 atoms	- -	9.75	-	-	67.24
Hydrogen	6 ——	- -	0.75	-	-	5.16
Oxygen	4 ——	- -	4.00	-	-	27.60
			14.50			100.00

Of crystalline citric acid, the prime equivalent is 8.375 by my experiments; and it consists probably of

Carbon	4 atoms	3.000	35.8
Hydrogen	3 ——	0.375	4.5
Oxygen	5 ——	5.000	59.7
		8.375	100.0

or, of 4 atoms carbon, 3 water, and 2 oxygen. 2 of these atoms of water are separated, when citric acid is combined with oxide of lead in what is called the dry citrate. Hence, the acid atom is in this case 6.125.

The prime equivalent of crystalline tartaric acid is 9.25 by my results; and it seems made up of

Carbon	4 atoms	3.0	32.43
Hydrogen	2 ——	0.25	2.70
Oxygen	6 ——	6.00	64.87
		9.25	100.00

or, of

Carbon	4 atoms	3.0	32.43
Oxygen	4 ——	4.0	43.24
Water	2 ——	2.25	24.33
		9.25	100.00

From my experiments I have been led to conclude, that into dry tartrate of lead these two atoms of water *do* enter as a constituent; and, hence, that the crystals of tartaric acid are as dry as is compatible with its constitution.

Oxalic acid crystals have 7.875 for their prime equivalent; and are composed of

Carbon	2 atoms	1.500	19.04
Hydrogen	3 ——	0.375	4.80
Oxygen	6 ——	6.000	76.16
		7.875	100.00

or, of 2 atoms Carbon	. .	1.5	. . 19.14
3 —— Oxygen	. .	3.0	. . 40.72
3 —— Water	. .	3.375	. . 40.14
		7.875	100.00

Into the dry oxalate of lead these 3 atoms of water do *not* enter. Hence, I find the dry acid to be composed of

Carbon 2 atoms	. .	1.5	. . 33.33
Oxygen 3 ——	. .	3.0	. . 66.66
		4.5	100.00

or, of 1 atom carbonic acid + 1 atom carbonic oxide, as was first suggested, I believe, by DOBEREINER. Crystallized oxalate of ammonia consists of 1 atom acid, 1 atom ammonia, and 2 atoms water = 8.875. By a gentle heat 1 atom of water may be separated; and an oxalate of ammonia, as dry as is compatible with its neutrality, remains.

I have analyzed, by the peroxide of copper, the citrate, tartrate, and oxalate of lead; and on comparing the results thus obtained, with those derived from the analysis of the crystalline acids, I have come to the above determinations.

Ferroprussic acid, the ferrocyanic acid of the French chemists, has proved, hitherto, a stumbling block to me, in reducing the results of my experiments to the atomic theory. I have subjected it to very numerous trials in many states of combination, and have sought, with great pains, to accommodate the results to the doctrine of prime equivalents; but hitherto without success. The following facts, however, may perhaps be deemed of some consequence.

In the first place, the prime equivalent of the crystallized ferroprussiate of potash is 13.125, compared to oxide of lead

14, and to nitrate of the same metal 20.75; that is, 13.125 of the former salt neutralize 20.75 of the latter. In the second place, 14 parts of oxide of lead yield 21 parts of dry ferroprussiate of lead; or the atomic weight of dry ferroprussic acid is 7.

The mean of my analyses of ferroprussiate of lead, gives the relation of the constituents of the acid, as marked in the table. These proportions, reduced to the atomic weight 7, afford

Carbon . .	2.5774
Azote . . .	2.4703
Ferreous matter	1.9523
	7.0000

Were we to suppose the prime equivalent of the ferroprussic acid 7.5 instead of 7; and were we farther to suppose that the carbon in the above result should be 2.25 = 3 atoms, and the azote = 3.5, or two atoms, then we might conceive an atom of dry ferroprussic acid to be made up of

Carbon	3	atoms	2.25
Azote	2	——	3.50
Iron	1	——	1.75
			7.50

But experiment does not permit me to adopt this theoretical representation.

The best mode that has occurred to me for analyzing ferroprussiate of potash, is to convert it, by the equivalent quantity of nitrate of lead, into the ferroprussiate of this metal; then to separate the nitrate of potash by filtration; and, after evaporation, to determine its weight. In this way,

13.125 grains of crystallized ferroprussiate of potash afford 12.33 grains of nitre, which contain 5.8 of potash.* By heating nitric acid in excess on 21 grains of ferroprussiate of lead, I obtained 2.625 grains of peroxide of iron, equivalent to 1.8375 of the metal. Hence I infer, that the iron in the ferroprussiate of lead is in the metallic state; for the joint weights of the carbon and azote contained in 7 grains of the dry acid is 5.0477; and the difference, 1.9523, approaches too closely to the above quantity, 1.8375, for us to suppose the metal to be in the state of protoxide. In fact, 2.625 parts of peroxide $\times$ 0.9 = 2.3625 of protoxide, is a quantity much beyond what experiment shows to be present.

* By careful desiccation, 1.69 grains of water, may be separated from 13.125 grains of the salt.

PRESENTS

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1821.	PRESENTS.	DONORS.
Nov. 8.	The Statutes of the Realm, printed by Command of H. M. King George the III. in pursuance of an Address of the House of Commons of Great Britain. Vol. VIII. 1821. F°	The Commissioners of Public Records.
	The Asiatic Researches, or Transactions of the Society instituted in Bengal, for inquiring into the History and Antiquities, the Arts, Sciences, and Literature, of Asia. Vol. XIII. Calcutta, 1820. 4°	The Asiatic Society.
	A Dictionary of the Chinese Language. Vol. I. Part 4, by R. Morrison, D.D. Macao, 1820. 4°	The Court of Directors of the Honourable East India Company.
	Mémoires de l'Académie Imperiale des Sciences de St. Petersbourg. Vols. V. VI. and VII. St. Petersburgh, 1815, 1818, 1820. 4°	The Imperial Academy of St. Petersburgh.
	The Universal Cambist and Commercial Instructor, being a full and accurate Treatise on the Exchanges, Monies, Weights and Measures of all trading Nations and their Colonies, with an Account of their Banks, Public Funds, and Paper Currencies, by P. Kelly, LL.D. 2d Edition. London, 1821. 2 Vols. 4°	Dr. P. Kelly.
	An Account of the means which have been taken to preserve the British Navy, from the earliest period to the present time, particularly from that species of Decay now denominated Dry Rot, by John Knowles. London, 1821. 4°	John Knowles, Esq.
	Memoirs of the American Academy of Arts and Sciences. Vol. IV. Part 2. Cambridge, 1821. 4°	The American Academy of Arts and Sciences.
	Mémoire sur la Distribution de la Chaleur dans les Corps solides. 4°	M. Poisson.

1821.	PRESENTS.	DONORS.
Nov. 8.	Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Vol. XI. part 2. London, 1821. 8°	The Medical and Chirurgical Society of London.
	The Principles and Doctrine of Assurances, Annuities on Lives, and Contingent Reversions, stated and explained, by William Morgan, F.R.S. London, 1821. 8°	William Morgan, Esq.
	A Compendious Treatise on the Theory and Solution of Cubic and Biquadratic Equations, and of Equations of the higher order, by the Rev. B. Bridge, B.D. F.R.S. London, 1821. 8°	Rev. B. Bridge.
	The proximate Causes of material Phænomena, and the true Principles of universal Causation considered and illustrated, by Sir Richard Phillips. London, 1821. 12°	Sir Richard Phillips.
	A Collection of Tracts. 8°	Dr. Andrew Duncan.
	The Nautical Almanac, and Astronomical Ephemeris for the year 1824.	The Commissioners of Longitude.
	The Works of Confucius, in Chinese. 5 Parts. 8°	Mr. William Huttman.
	Voyages dans la Grande Bretagne entrepris relativement aux Services publics de la Guerre et de la Marine, et des Ponts et Chaussées, en 1816-1819, par Ch. Dupin. Paris, 1820. 4 Vols. 4°	M. Dupin.
15.	Flora Batava. No. 59.	H. M. The King of the Netherlands.
	The Modern Church, a Satirical Poem, comprising Sketches of some popular and unpopular Preachers, by John Laurens Bicknell, F. A. S. London, 1820. 8°	John Laurens Bicknell, Esq.
22.	Vetus Testamentum Græcum a Codice MS. Alexandrino qui Londini in Bibliotheca Musei Britannici asservatur Typis ad similitudinem ipsius Codicis Scripturæ fideliter descriptum curâ et labore H. H. Baber, A. M. Vol. 1. London, 1816. F°	The Trustees of the British Museum.
	A Natural History of the Crinoeidea or Lilly-shaped Animals, with Observations on the Genera Asteria, Euryale, Comatula and Marsupites, by J. S. Miller. Bristol, 1821. 4°	Mr. J. S. Miller.
	An Account of the Animals seen by the late Northern Expedition, whilst within the Arctic Circle, by Captain Edward Sabine, R. A. London, 1821. 4°	Captain Edward Sabine.
	Raguaglio del Reale Osservatorio di Napoli eretto sulla Collina di Capodimonte 4°	M. Piazzi.
	Ephemeris of the Distances of the four Planets, Venus, Mars, Jupiter, and Saturn, from the Moon's Centre, for 1822, to which is annexed, Tables for finding the Latitude of the Polar Star for 1821-1822, by H. C. Schumacker. Copenhagen, August, 1820. 4°	Professor Schumacker.
	Astronomische Hülfstafeln für 1821, von H. C. Schumacker. Copenhagen. 8°	———

1821.	PRESENTS.	DONORS.
Nov. 22.	An additional Plate and Sheet to the History of the Soke of Grantham.	Edmund Turnor, Esq.
Dec. 6.	Transactions of the Geological Society, Vol. V. Part 2. London, 1821. 4°	The Geological Society.
	Annales des Mines ou Recueil des Mémoires sur l'Exploitation des Mines et sur les Sciences qui s'y rapportent, redigées par le Conseil Général des Mines. No. 2 and 3 for 1821. 8°	Le Conseil Général des Mines.
	Of Population. An Inquiry concerning the Power of Increase in the Numbers of Mankind, being an Answer to Mr. Malthus' Treatise on that Subject, by W. Godwin. London, 1820. 8°	William Godwin, Esq.
	The Introductory Lecture of a Course upon State Medicine, by John Elliotson, M. D. London, 1821. 8°	John Elliotson, M. D.
	Lezioni Elementari di Cosmografia, di Geografia, e di Statica di Jacopo Gråberg di Hemso. Genoa, 1819, 8°.	M. J. Gråberg di Hemso.
	Lettera del Sig Gråberg di Hemso all' Illus. Sig. L. Gropi sulla Peste di Tangeri negli anni 1818-19. Genoa, 1820. 8°	———————
1822.		
Jan. 10.	Histoire Naturelle et Medicale des Casses, et particulièrement de la Casse et des Sénés employés en Medécine par L. T. F. Colladon, M.D. Montpellier, 1816. 4°	Dr. Colladon.
	Narrative of a Descent in the Diving Bell, by Dr. Louis Theodore Frederick Colladon. 8°	———————
	Introduction to a synoptical Table of English and French Lineal Measures. 4°	Mr. Goodwyn.
	Astronomical Tables and Remarks for the Year 1822, by Francis Baily, F. R. S. London, 1822. 8°	Francis Baily, Esq.
	Meteorological Journal kept on board the Marine Society's Ship in the Year 1821.	The Committee of Governors of the Marine Society.
	Poesias Lyricas de Francisco de Borja Garçao Stockler. Londres, 1821. 8°	M. F. B. G. Stockler.
	Observations et Reflexions concernant la Polarité de la Chaleur. 4°	M. Henri de Schuzler.
17.	A Collection of Examples of the Application of the Differential and Integral Calculus, by George Peacock. Cambridge, 1820. 8°	Mr. Peacock.
	A Collection of the Examples of the Applications of the Calculus of Finite Differences, by J. F. W. Herschel. Cambridge, 1820. 8°	J. F. W. Herschel, Esq.
24.	Memoir of the late Captain Joseph Huddart, F. R. S. London, 1821. 4°	Sir Joseph Huddart.
	Chart of the Southern part of the China Sea. And Chart of the Straits and Islands at the South-west part of the China Sea.	James Horsburgh, Esq.
31.	Calendarium Inquisitionum post Mortem sive Escætarum. Vol. III. Temp. Ric. 2 et Hen. 4, 1821. F°	The Commissioners of Public Records.

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Jan. 31.	Valor Ecclesiasticus Temp. Henr. VIII. Auctoritate Regiâ institutus. Vol. IV. 1821. F°	The Commissioners of Public Records.
	An Introductory Discourse delivered to the Leeds Philosophical and Literary Society, April 6, 1821, by Charles Turner Thackrah. Leeds. 4°	C. T. Thackrah, Esq.
	Transactions of the Society Instituted at London for the Encouragement of Arts, Manufactures, and Commerce. Vol. XXXIX. London, 1821. 8°	The Society of Arts.
Feb. 7.	Mémoire sur la Combinaison de l'Oxigène avec l'Eau et sur les Propriétés extraordinaires que possède l'Eau oxigenée, par M. Thenard. 4°	M. Thenard.
	Observations sur la Physiologie Végétale et sur la Système physiologique de M. Aubert du Petit Thouars, par M. Feburier. 8°	M. Feburier.
	Rapport sur les Moyens proposés jusqu'à ce jour pour préserver les Blés de la Carie, par M. Feburier. 8°	————
	The Chemical Catechism, with Tables, Notes, Illustrations, and Experiments, by Samuel Parkes. London, 1822. 8°	Samuel Parkes, Esq.
21.	Memoirs of the Board of Agriculture of the State of New York. Vol. I. Albany, 1821. 8°	Dr. David Hosac.
	Narrative Journal of Travels through the North-west Regions of the United States, extending from Detroit through the great Chain of American Lakes to the Sources of the Missisippi River, performed as a Member of the Expedition under Governor Cass in the year 1820, by Henry R. Schoolcraft. Albany, 1821, 8°	————
	Observations Physico-Chimiques sur les Alliages du Potassium et du Sodium avec d'autres Métaux, Propriétés de ces Alliages, &c. par G. S. Serullas. Metz. 1820. 8°	M. G. S. Serullas.
	Memorie della Reale Accademia delle Scienze di Torino. Tomo 24. Turin, 1820. 4°	Royal Academy of Sciences of Turin.
	Transactions of the Horticultural Society of London. Vol. IV. Part 4.	The Horticultural Society.
	An humble Memorial of King George III. at Longford Castle, Wilts, (an Engraving).	The Right Honourable The Earl of Radnor.
March 7.	Fœdera, Conventiones, Litteræ, et cujuscunque Acta publica inter Reges Angliæ et alios quosvis Imperatores, Reges, Pontifices, Principes vel Communitates, ab ingressu Gulielmi I^mi. in Angliam A.D. 1066 ad nostra usque tempora habita aut tractata. Vol. II. Part 2. London, 1821. F°	The Commissioners of Public Records.
14.	Esquisse du Règne Vegétale ou Tableau caracteristique des Familles des Plantes, par A. L. Marquis. Rouen, 1820. 8°	M. Marquis.
	Fragmens de Philosophie Botanique ou de la Manière la plus convenable de voir et de travailler en histoire naturelle et particulièrement en Botanique, et des Moyens de rendre cette Science plus simple et plus facile, par A. L. Marquis. 1821. 8°	————

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	On a new Compound Gas resulting from Animal decomposition taking place in the living Body, with some general remarks on Tympanitis. 1822. 8°	Dr. A. B. Granville.
21.	Histoire de l'Astronomie Moderne, par M. Delambre. Paris, 1821. 2 Vols. 4°	M. Delambre.
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	Tracts on Vaults and Bridges. London, 1822. 8°	Mr. Samuel Ware.
	Memoirs of the Astronomical Society of London. Vol. I. London, 1822. 4°	The Astronomical Society.
	A New Method of Solving Equations with Ease and Expedition, by Theophilus Holdred. London, 1820. 4°	M. T. Holdred.
April 18.	Die Phosphorescenz der Körper oder die im Dunkeln bemerkbaren Lichtphænomene der anorganischen Natur—durch eine Rieke einiger Beobachtungen und Versuche geprüft und bestimmt von Placidus Heinrich. 4 part. Nuremberg, 1820. 4°	M. P. Heinrich.
	A Complete Course of practical Geometry, including Conic Sections, and Drawing, treated on a principle of peculiar perspicuity, by C. W. Pasley, Royal Engineers. London, 1822. 8°	Lieut. Col. C. W. Pasley.
	The Use of the Blow-pipe in Chemical Analysis, and in the Examination of Minerals, by J. J. Berzelius, translated from the French of M. Fresnel, by J. G. Children. London, 1822. 8°	J. G. Children, Esq.
	Annales des Mines. Part 4, for 1821. 8°	Le Conseil des Mines.
25.	On the Places of 145 new double Stars, by Sir W. Herschel. 4°	Sir William Herschel.
May 2.	The Hunterian Oration in Honour of Surgery, and in Memory of those Members by whose Labours it's celebrity has been advanced; instituted by the Executors of John Hunter; delivered in the Theatre of the College, February 14, 1822. London, 1822. 4°	Sir Everard Home, Bart.
9.	Beitrage zur chorographischen Kenntniss des Flussarbeits der innerste in der Furstenthumern Grubenhagen und Hildesheim, mit besonderer Rucksicht auf die Veranderungen die durch diesen Strom in der Beschaffenheit des Bodens und in der Vegetations bewerkt worden sind. Eine Erlst Anlage zur Flora des Konigreichs Hannover, von G. F. W. Meyer. Gottingen, 1822. 2 Vols. 8°	M. G. F. W. Meyer.
	Remarks on the present defective State of the Nautical Almanac, by Francis Baily. London, 1821. 8°	Francis Baily, Esq.

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May 16.	Memoirs of the Imperial Academy of St. Petersburgh. Vols. I. II. III. and IV. In the Russian Language. 3 Vols. 8°.	The Imperial Academy of St. Petersburgh.
	Practical Observations on the Nautical Almanac and Astronomical Ephemeris, by James South, Esq. F.R.S. London, 1822. 8°	James South, Esq.
23.	Statutes of the Realm. Vol. IX. 1822. F°	The Commissioners of Public Records.
	Supplement to the IVth Volume of the Transactions of the Horticultural Society. London, 1822. 4°	The Horticultural Society.
	Reasons why the Bank of England ought not to reduce the Rate of Discount to 4 per cent., by an Impartial Observer. 2nd Edition. London, 1822. 8°	The Author.
	Exposition of the Real Causes and Effective Remedies of the Agricultural Distress, by an Impartial Looker-on. London, 1822. 8°	———
	The Agricultural Case and Cash Payments briefly considered in a Letter from the Continent to a Friend in England. London, 1822. 8°	———
	A New Theory of the Tides, shewing what is the immediate Cause of the Phænomenon, and which has hitherto been overlooked by Philosophers, by Captain Forman, R. N. London, 1822. 8°	Captain Forman.
June 6.	Travels in Syria and the Holy Land, by the late John Lewis Burckhardt, published by the Association for promoting the Discovery of the Interior Parts of Africa. London, 1822. 4°	The African Association.
	Outlines of the Geology of England and Wales, with an Introductory Compendium of the general Principles of that Science, and comparative View of the Structure of foreign Countries, by the Rev. W. D. Conybeare and William Phillips. Part 1. London, 1822. 8°	The Rev. W. D. Conybeare, and William Phillips, Esq.
	Transactions of the Cambridge Philosophical Society. Vol. I. Part 2. 4°	The Cambridge Philosophical Society.
	Discours sur la Lecture fondamentale des Langues, par le Conseiller de Cour Goulianoff. Paris, 1822. 8°	M. Goulianoff.
	Astronomical Observations made at the Radcliffe Observatory, Oxford, from the 1st May, 1821, to the 1st May, 1822. (MS.)	The Trustees under the will of the late Dr. Radcliffe.
	Wellington, Poemetto del Marchese Antonio Solari Veneziano;—and Wellington proved to be the greatest Warrior of Ancient and Modern Times, by Catherine Hyde Solari. London. 8°	The Marchioness Solari.
13.	Sermons, practical and occasional, Dissertations, Translations, including new Versions of Virgil's Bucolica, and of Milton's Defensio Secunda, Seaton Poems, &c. &c. by the Rev. Francis Wrangham. London, 1816. 3 Vols. 8°	Rev. Francis Wrangham.

1822.	PRESENTS.	DONORS.
June 13.	Ristretto di Fatti acaustici di Giovanni Dall' Armi letto in Accademia de' Lincei, Roma 1821. Edizioni litografica autografa. Fo. con Appendice letto già col Corredo degli Experimenti all'Accademia de' Lincei di Roma, ne' giorni 27 September e 4. Ottobre, 1821. Roma, Gennajo, 1822.	M. G. Dall' Armi.
20.	Observationes Medicæ ad Heberdeni Commentarios accommodatæ a Richardo Burney, A. B. Cambridge, 1822. 8°	Dr. Richard Burney.
	Carte de la Grèce Moderne dressée sur les Mémoires de M. Pouqueville et d'autres Voyageurs, et appuyée sur les Observations de plusieurs Navigateurs, par J. B. Barbie du Bocage, 1821. 1 sheet.	M. Barbie du Bocage.
27.	Star Tables, No. 2 for the year 1823, by Thomas Lynn, of the Honourable E. I. Company's Service, 1822. 8°	Mr. T. Lynn.
	Solar Tables for Working the Longitude by Chronometer, or for finding the Latitude by double Altitudes of the Sun or Stars. London, 1822. 8°	———
July 4.	Mémoire sur la Gelatine, par A. Michelot. Paris, 1822. 8°	M. Michelot.
	Testimonials presented to the Trustees of the British Museum in behalf of William Swainson.	William Swainson, Esq.
	The Naturalist's Guide for Collecting and Preserving subjects of Natural History and Botany, both in temperate and tropical countries, particularly Shells. London, 1822. 12°	———
	Message from the President of the United States, transmitting a Report of W. Lambert, on the Subject of the Longitude of the Capital of the United States. Washington, 1822. 8°	H. E. the American Ambassador.

Periodical Publications.

	A Journal of Science, Literature, and the Arts. No. 23, to No. 26. 8°	The Managers of the Royal Institution.
	Annals of Philosophy. New Series. No. 8 to 19. 8°	Richard Phillips, Esq.
	The Philosophical Magazine. No. 279 to 290. 8°	Dr. Alexander Tilloch.
	The Monthly Review enlarged, from July 1821, to June, 1822, with Appendices to Vols. XCV. XCVI. and XCVII.	Mr. G. E. Griffiths.
	The European Magazine, from July, 1821, to June, 1822. 8°	The Proprietors.
	The Monthly Censor. No. 1 for June, 1822.	Messrs. F. C. and J. Rivington.

In the List of Presents in the Philosophical Transactions for 1821, *page* 443,

For a Treatise on Heat, Flame, and Combustion, by T. H. Pasley. London, 1820. 8° Donor *Lieut. Col. Pasley,* read Donor *Lieut. T. H. Pasley.*

ERRATA.

Page 320, line 22, for *acid*, read *acids*.
—— 325, — 27, for *Pays*, read *Puy*.
—— 327, — 26, for *namely*, read *merely*.

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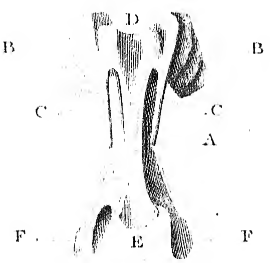


Fig. 2.

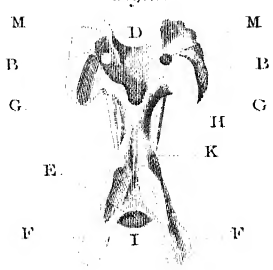


Fig. 3.

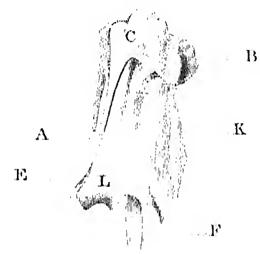
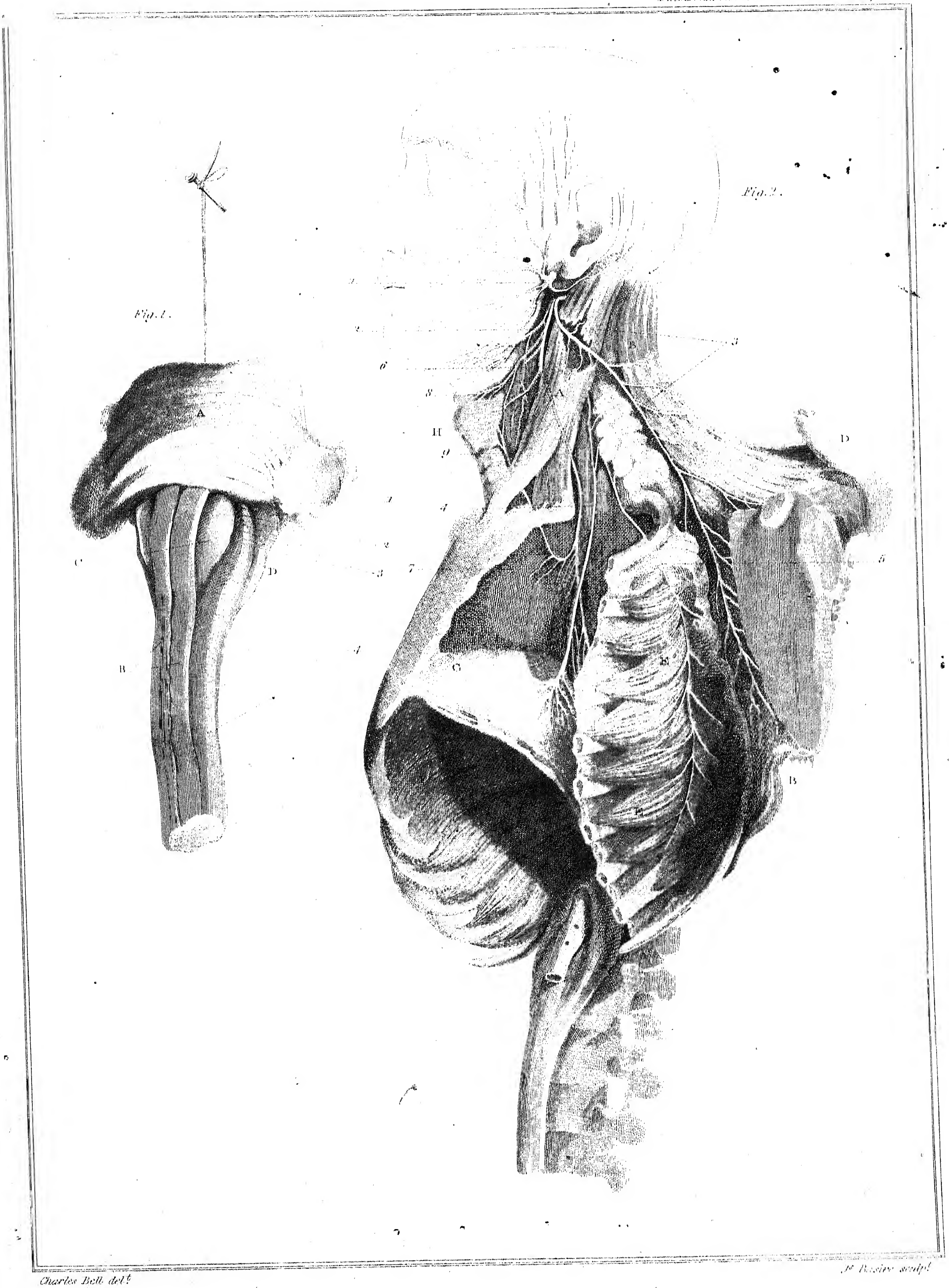


Fig. 4.

Fig. 5.

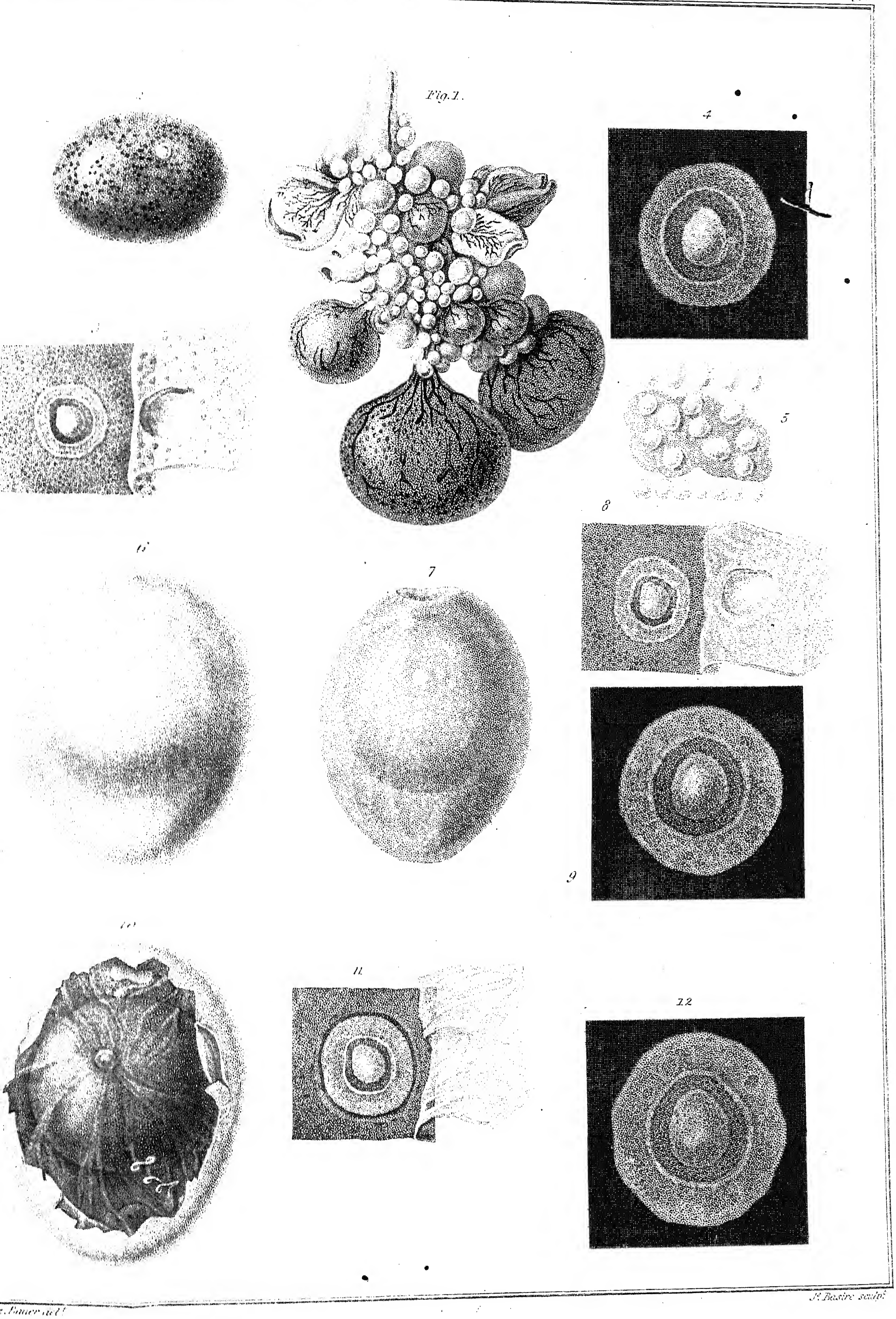
Fig. 6.

J. Basire del. et sculp.



Charles Bell del.t

J.s Basire sculp.t



.Bauer del. J. Basire sculp.

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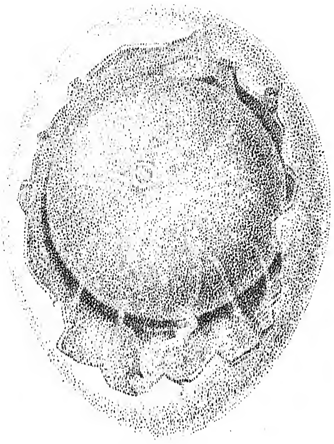

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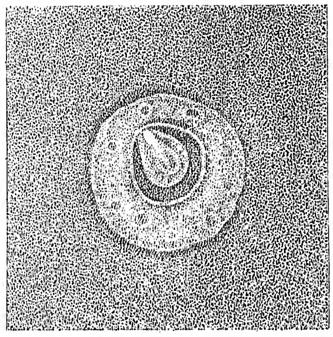

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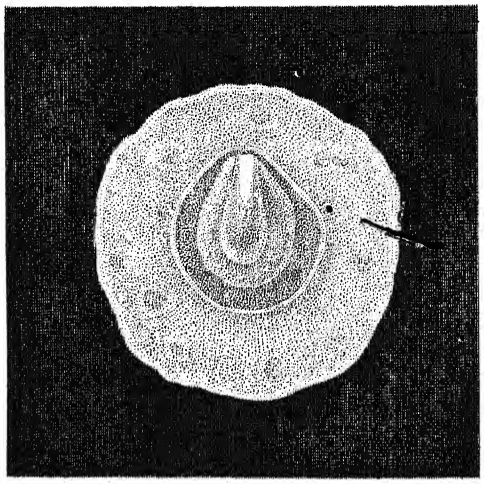

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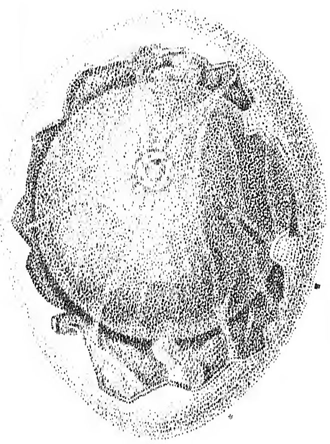

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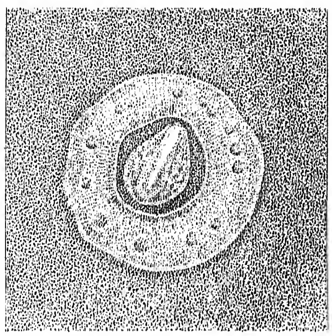

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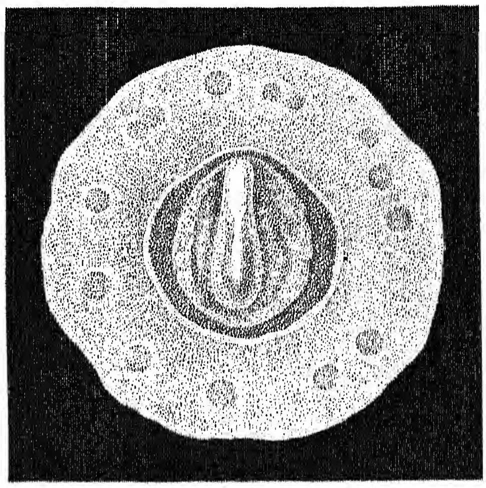

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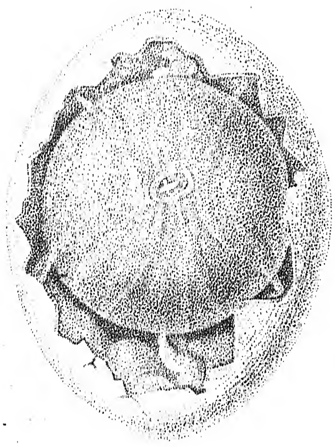

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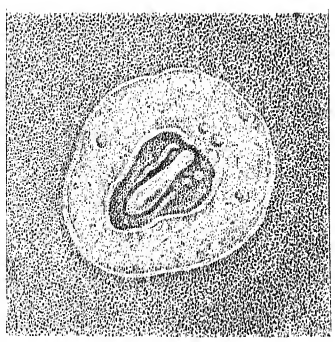

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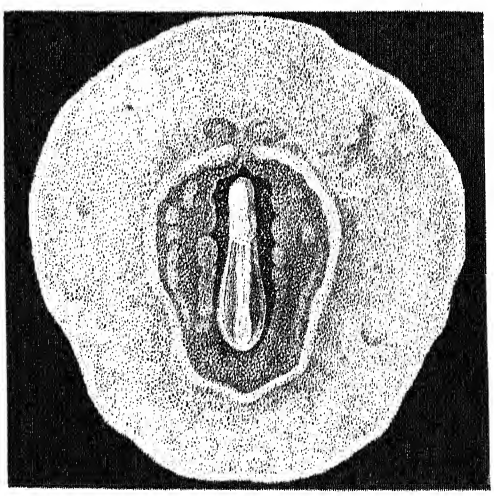
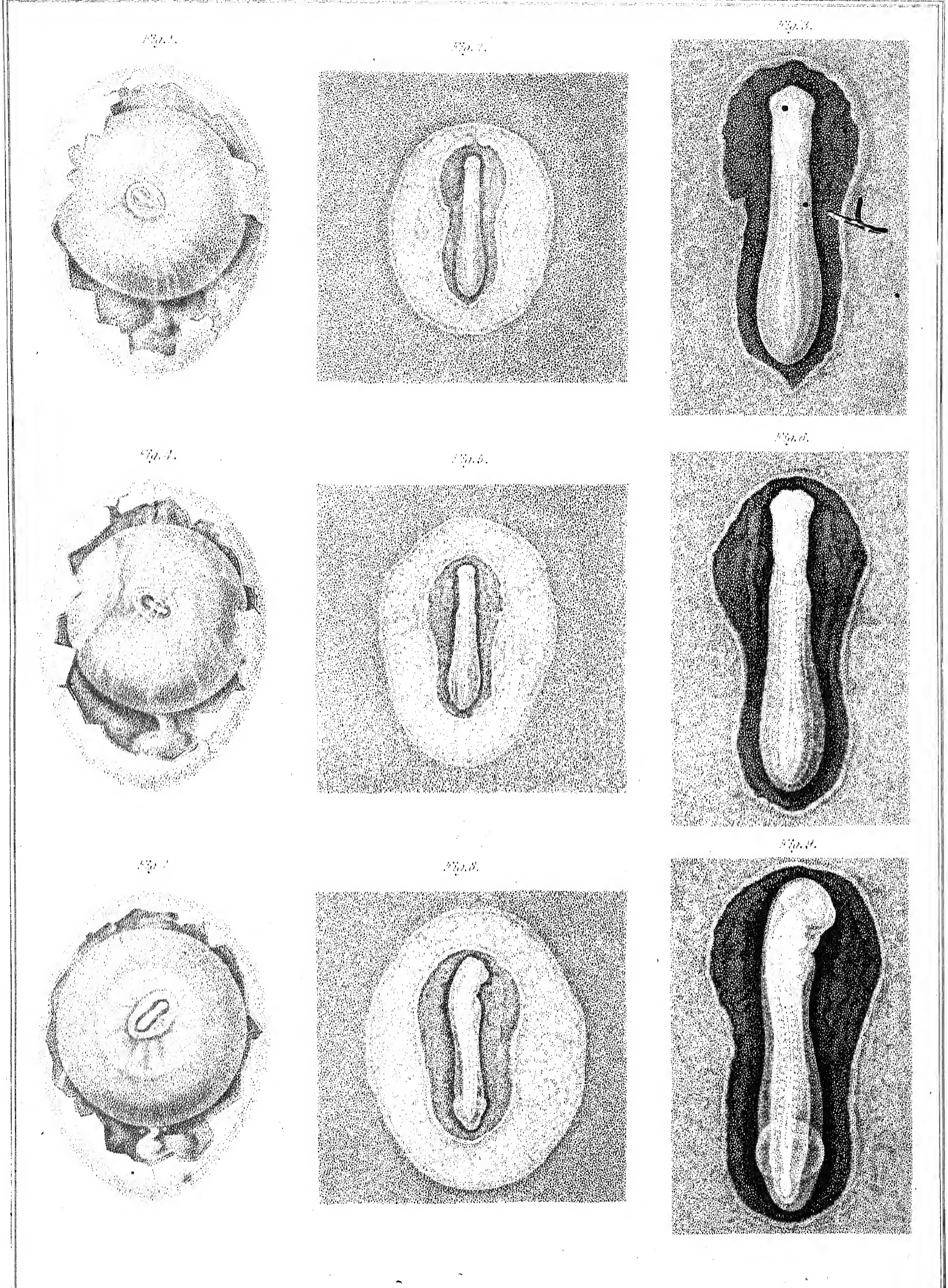


Franz Bauer del.

Jas Basire sculp.



Franz Bauer del.t J.s Basire sculp.t

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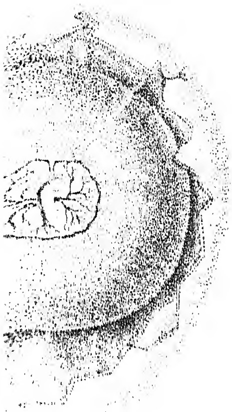

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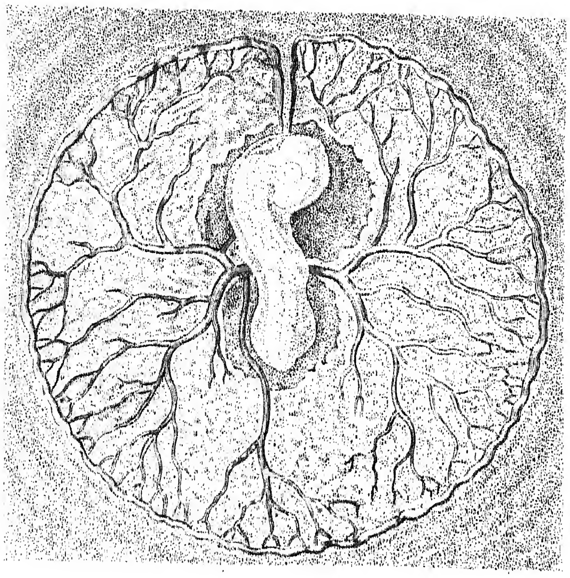

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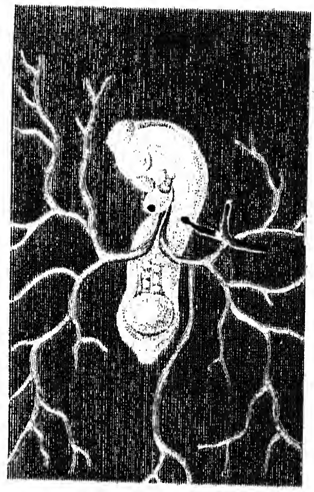

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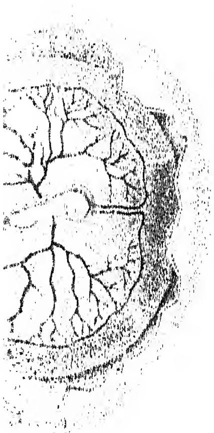

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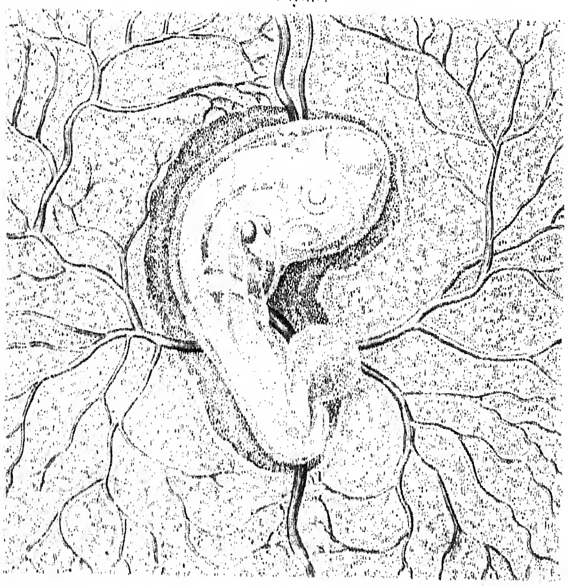

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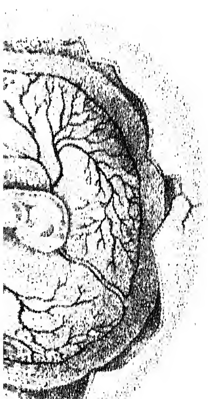

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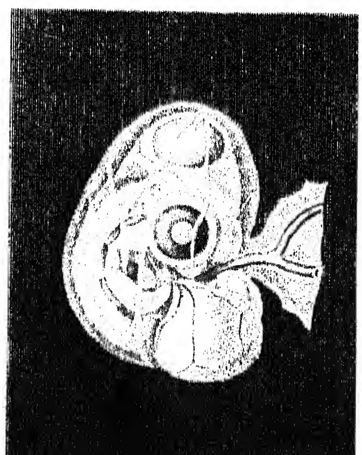
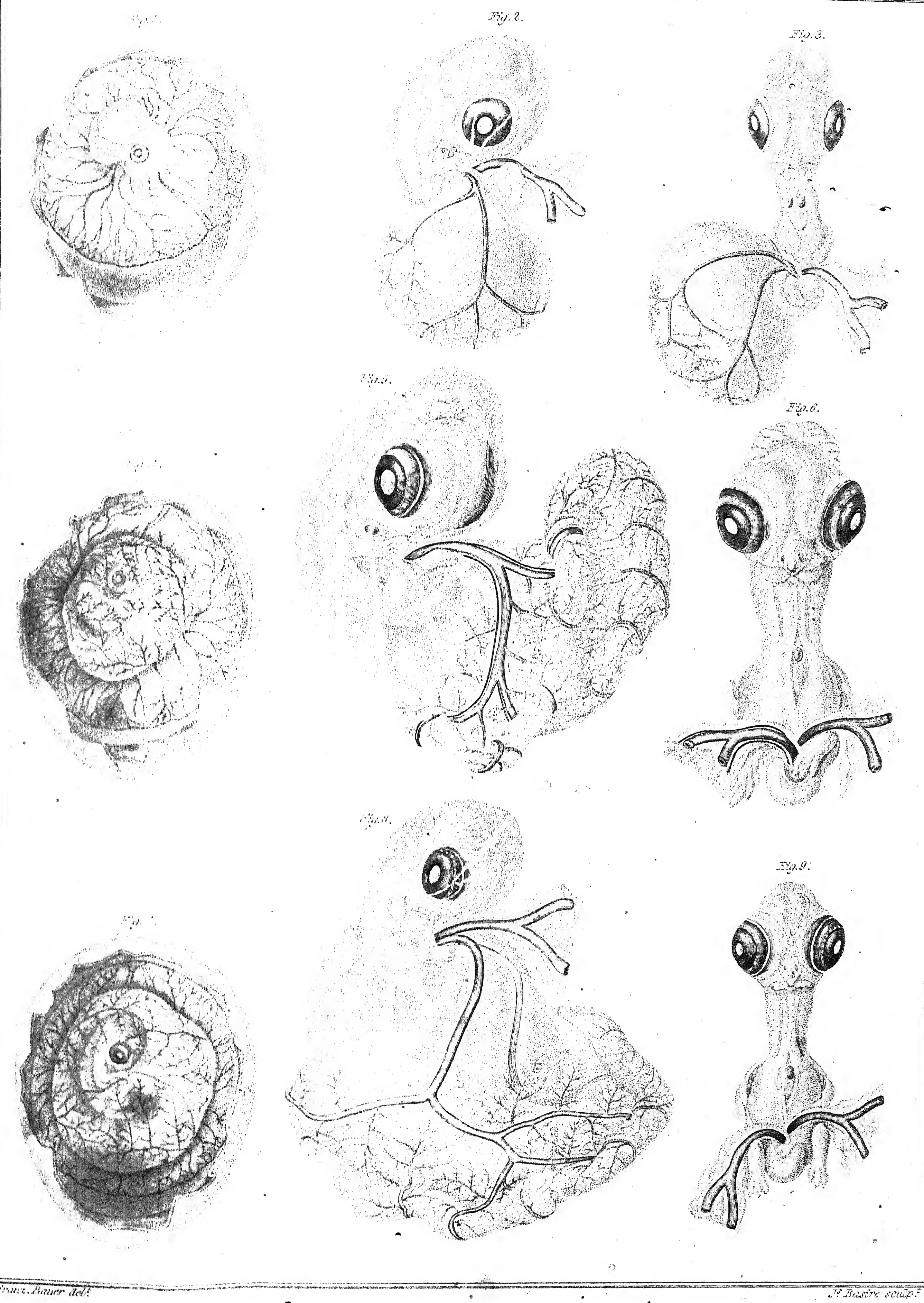


Fig. 9.

J. Basire sculp.



Franz. Bauer delt. J^s Basire sculp^t

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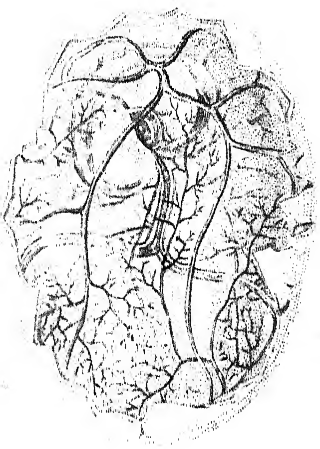

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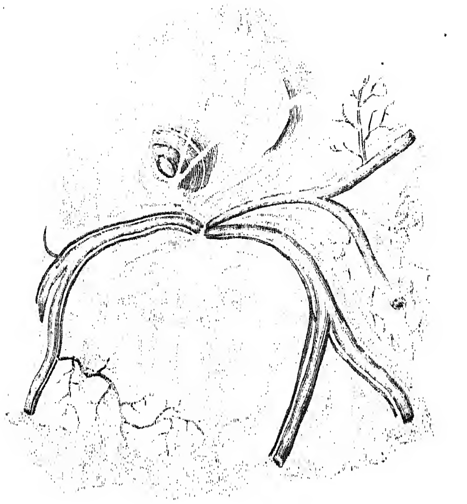

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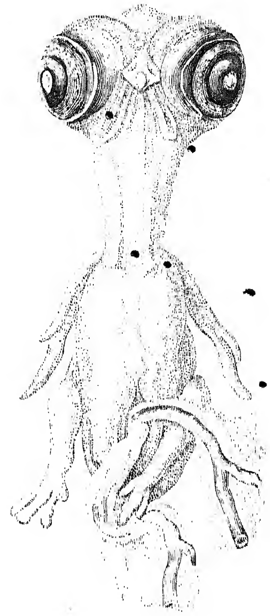

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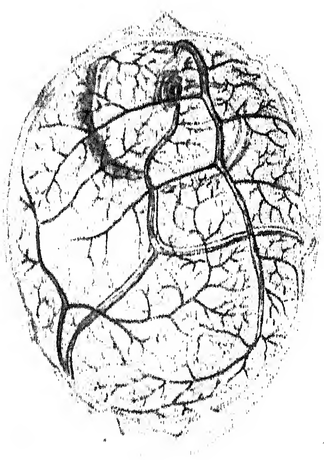

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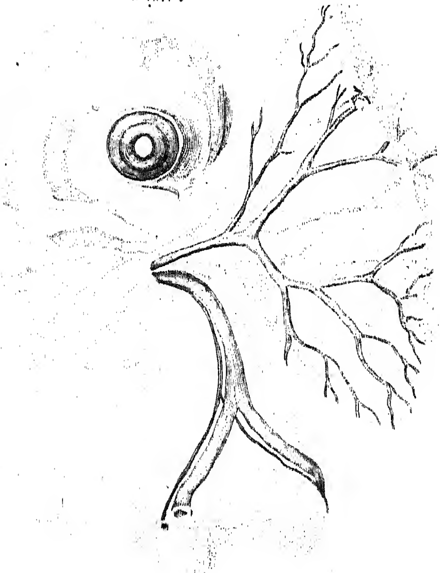

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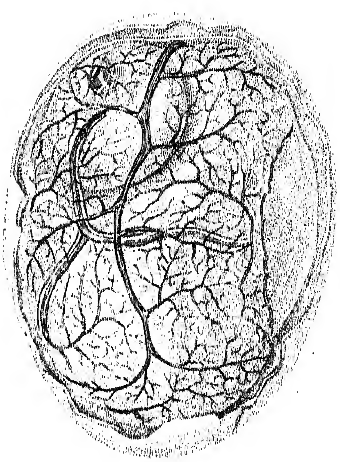

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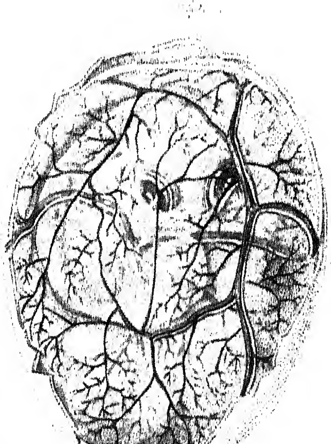

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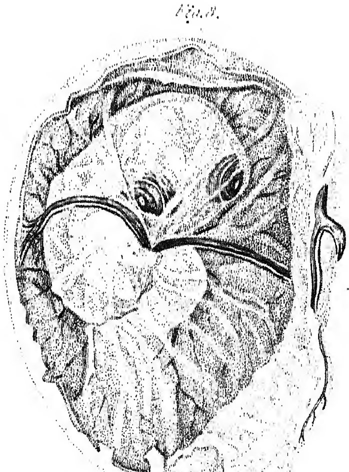
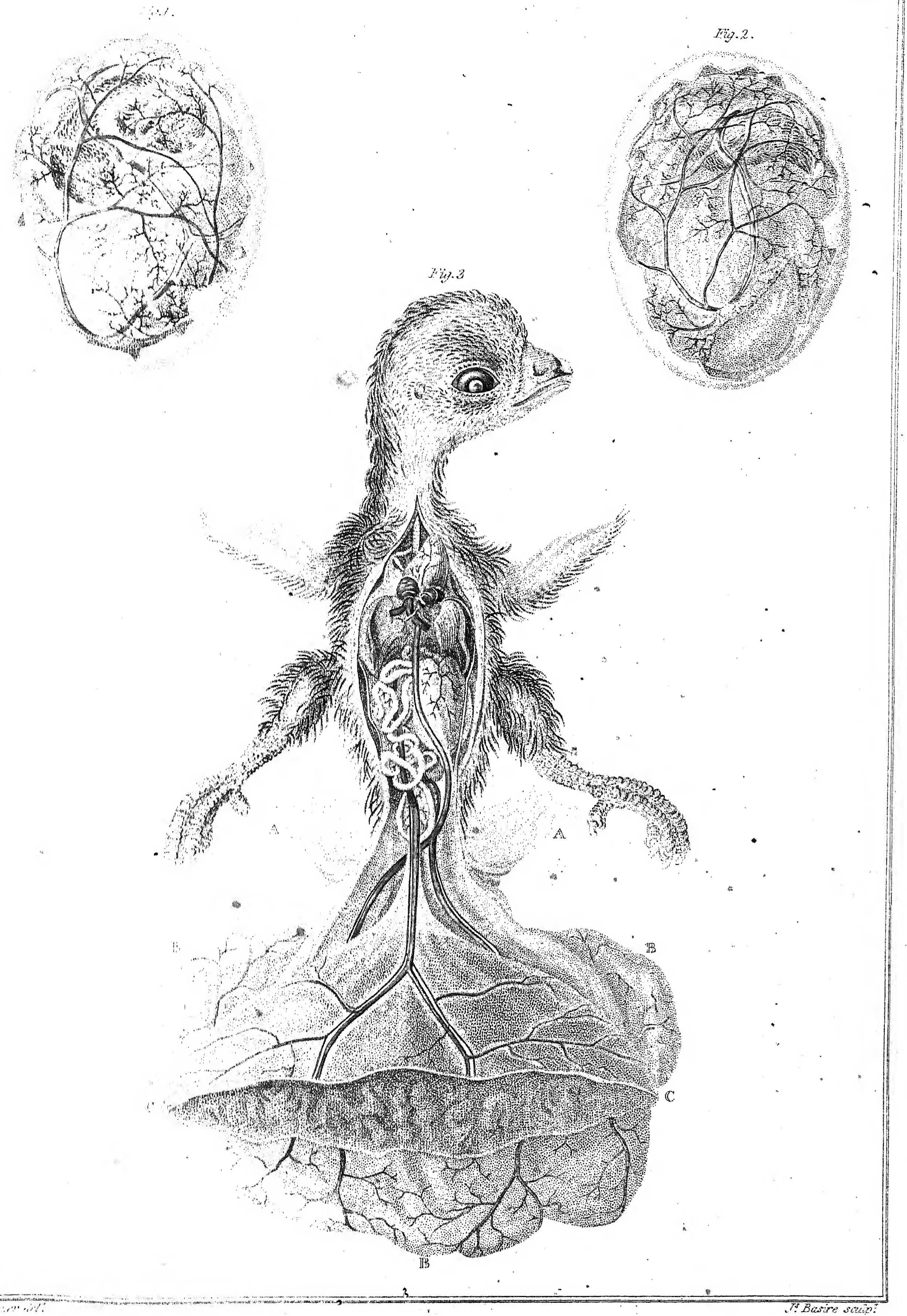


Fran. Bauer del.

J. Basire sculp.



del.

Js Basire sculpt.

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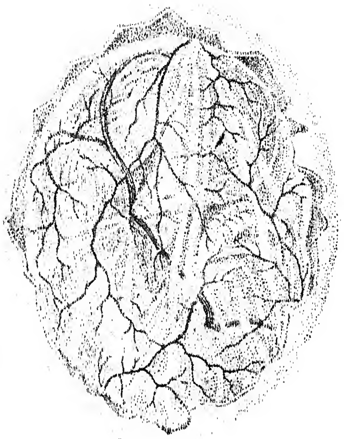

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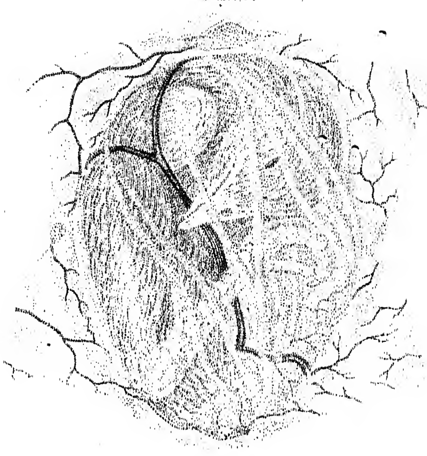

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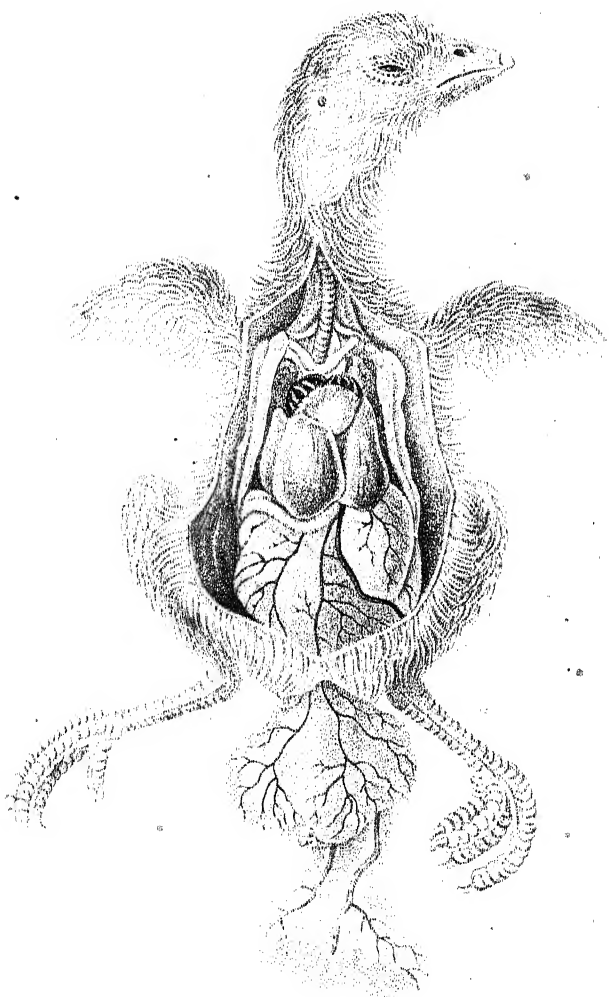
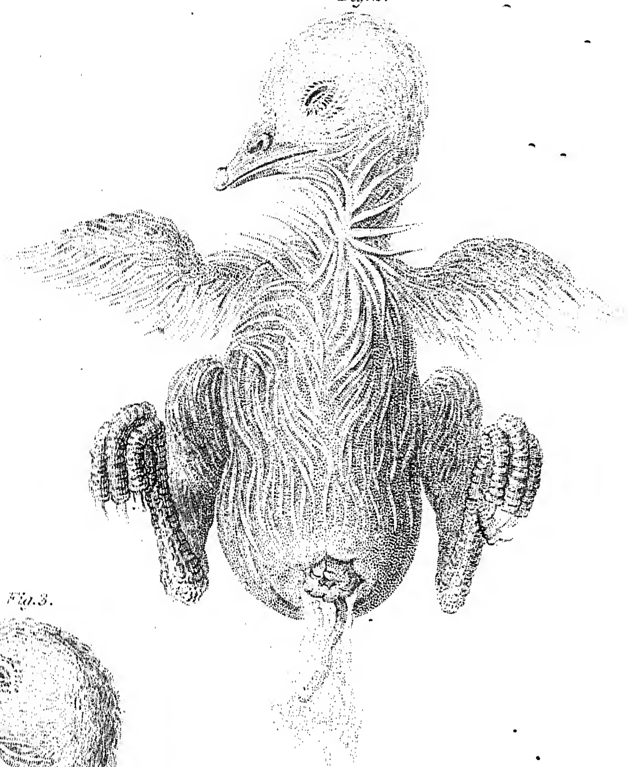


Franz Bauer del.

J. Basire sculp.

Fig. 2.



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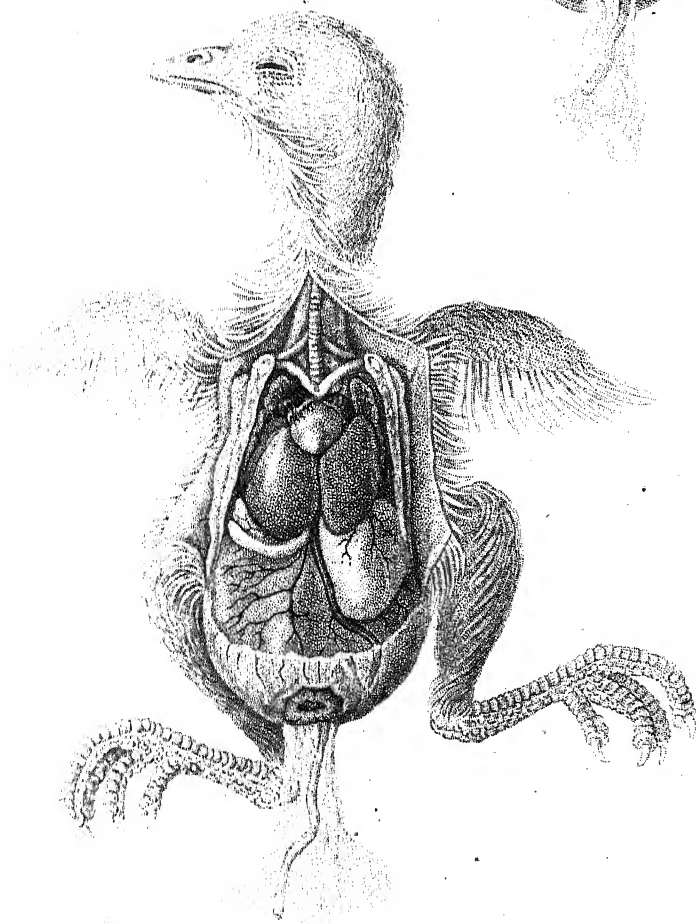
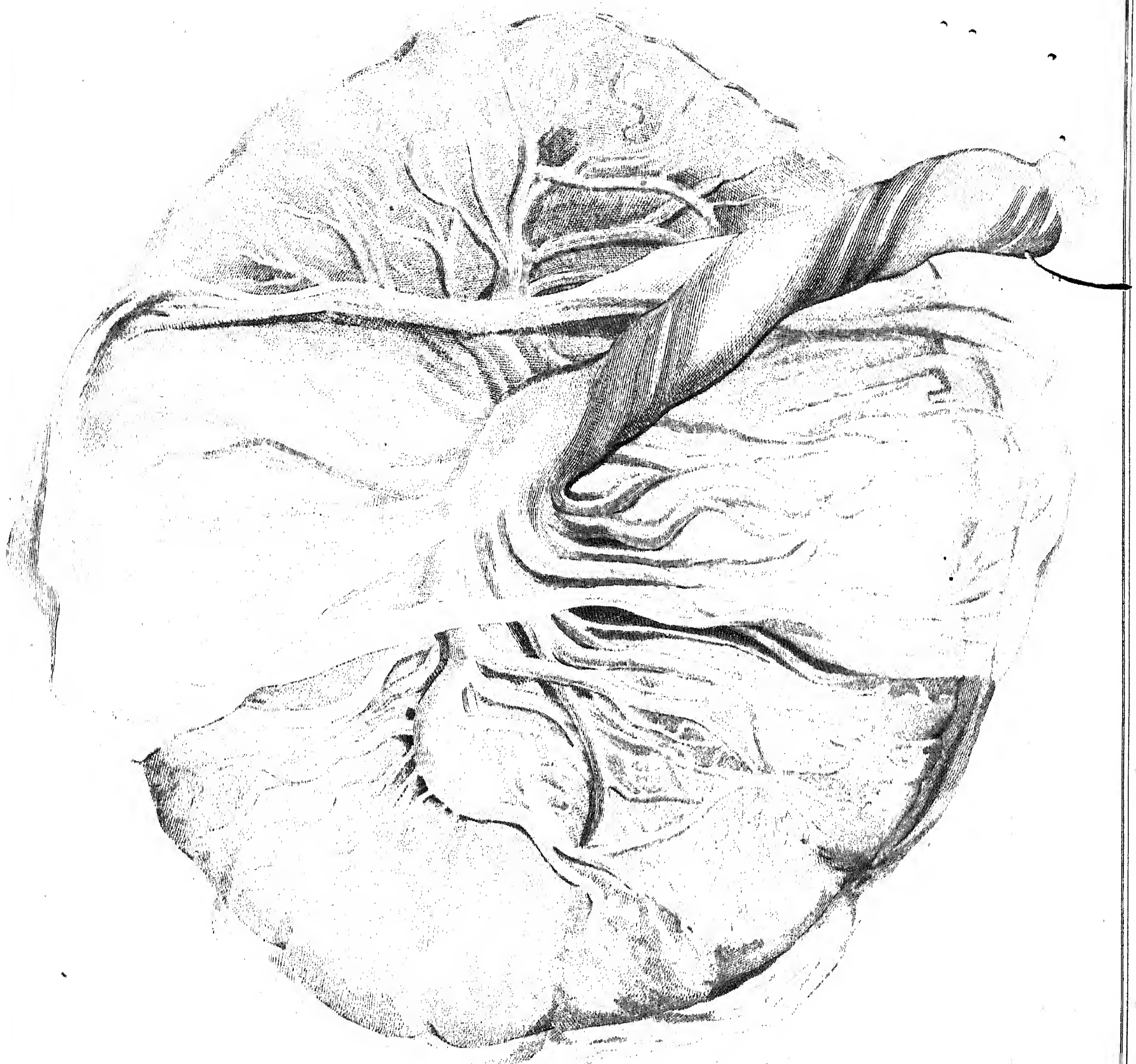
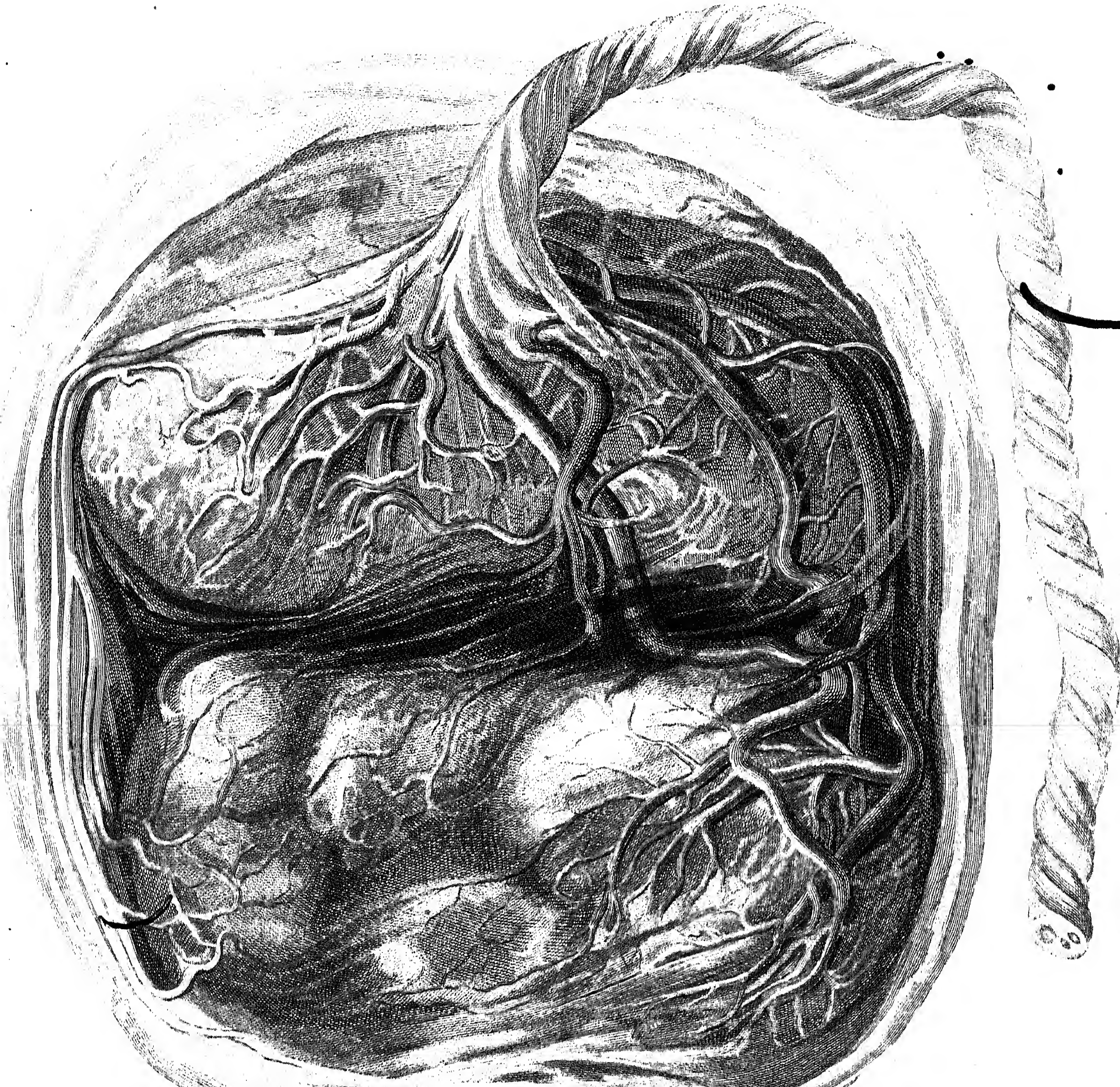




Wm Clift delt.

Js Basire sculpt.

Js. Basire sculpt.

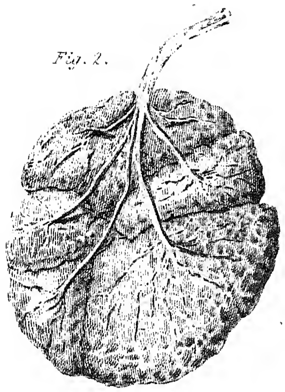


W^m. Clift del^t.   J. Basire sculp^t.

W^m Clift del^t. J^s Basire sculp^t.

Fig. 1.

Fig. 2.



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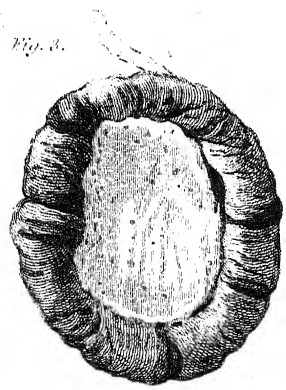

J. Basire sculp.

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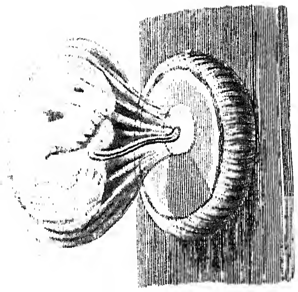

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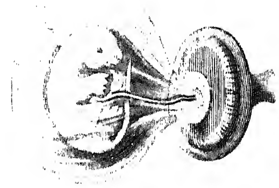

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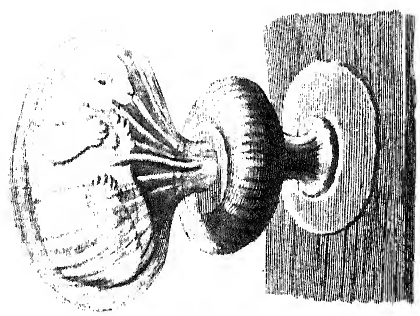
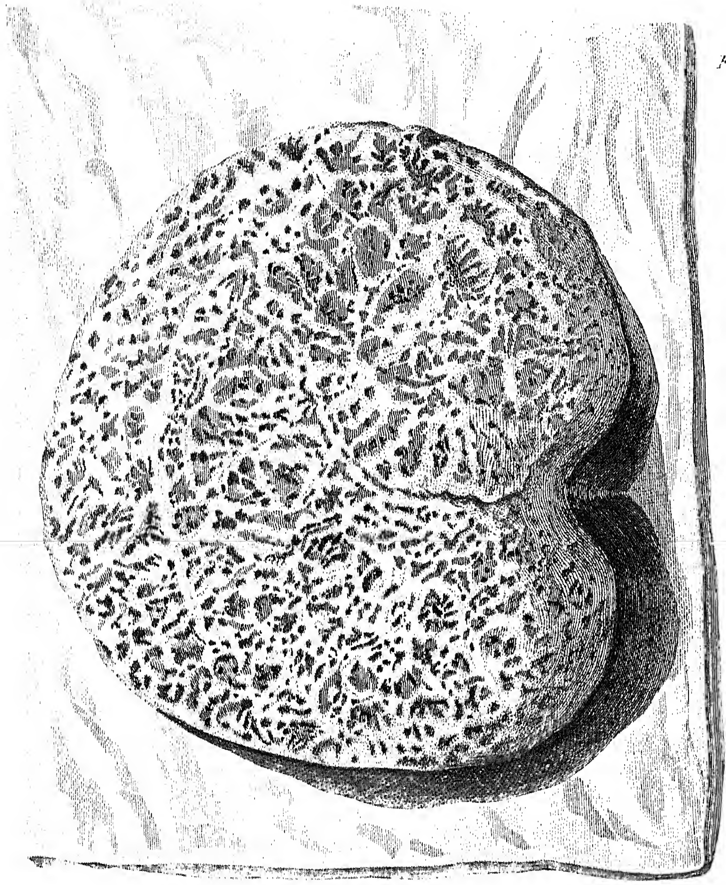


Wm Clift del. *J. Basire sculp.*

Fig. 2.



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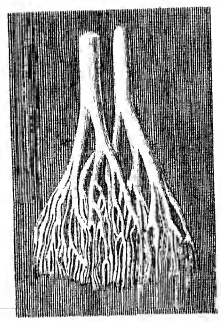

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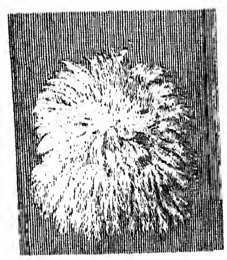

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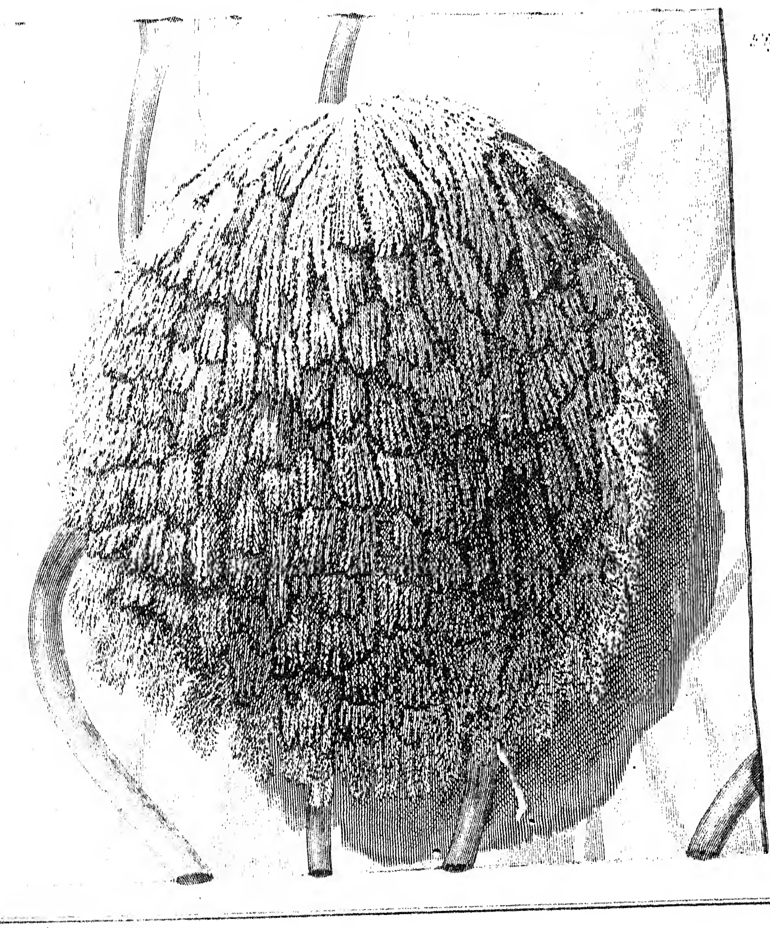

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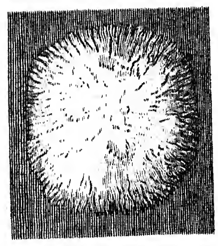

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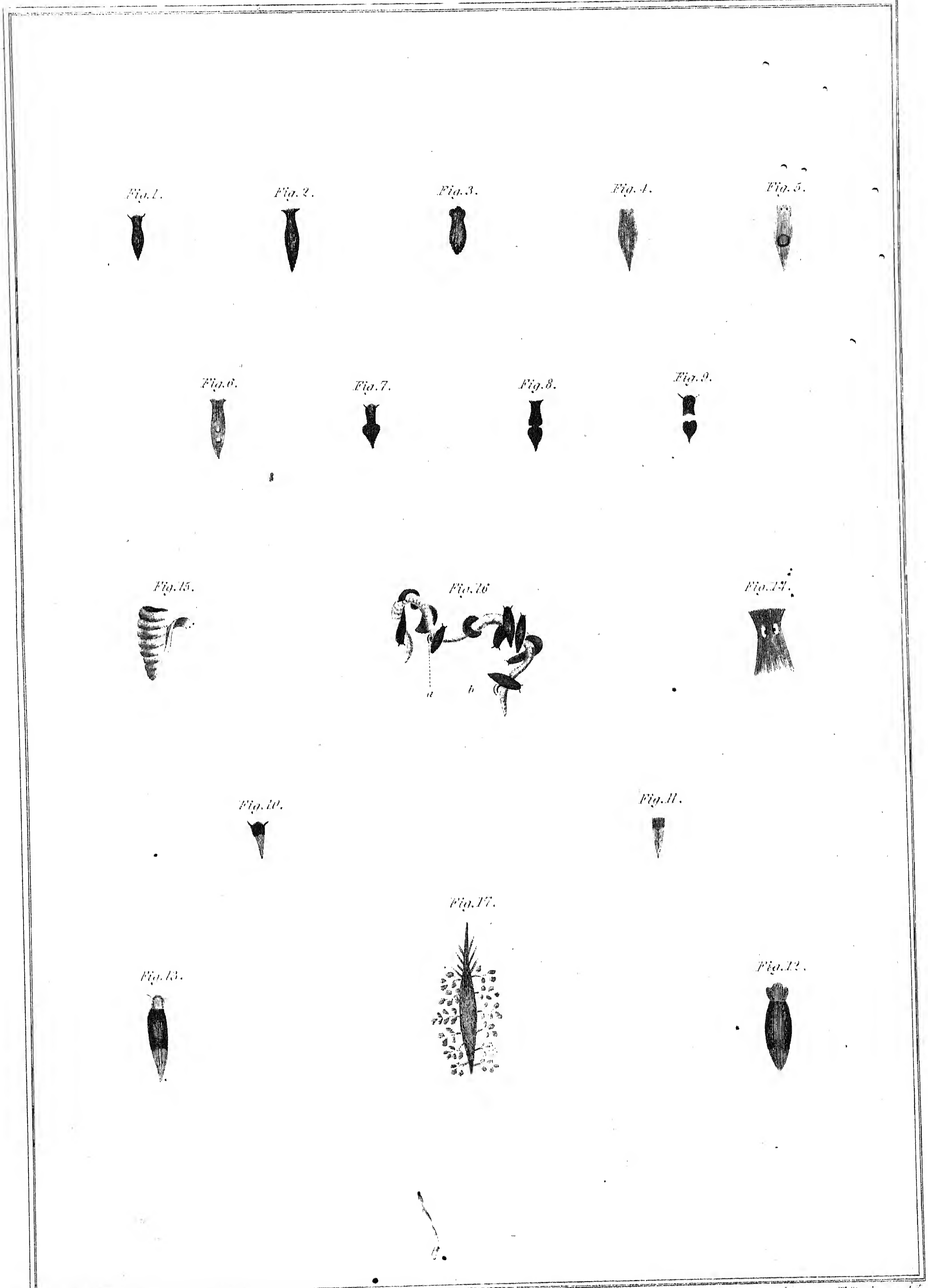

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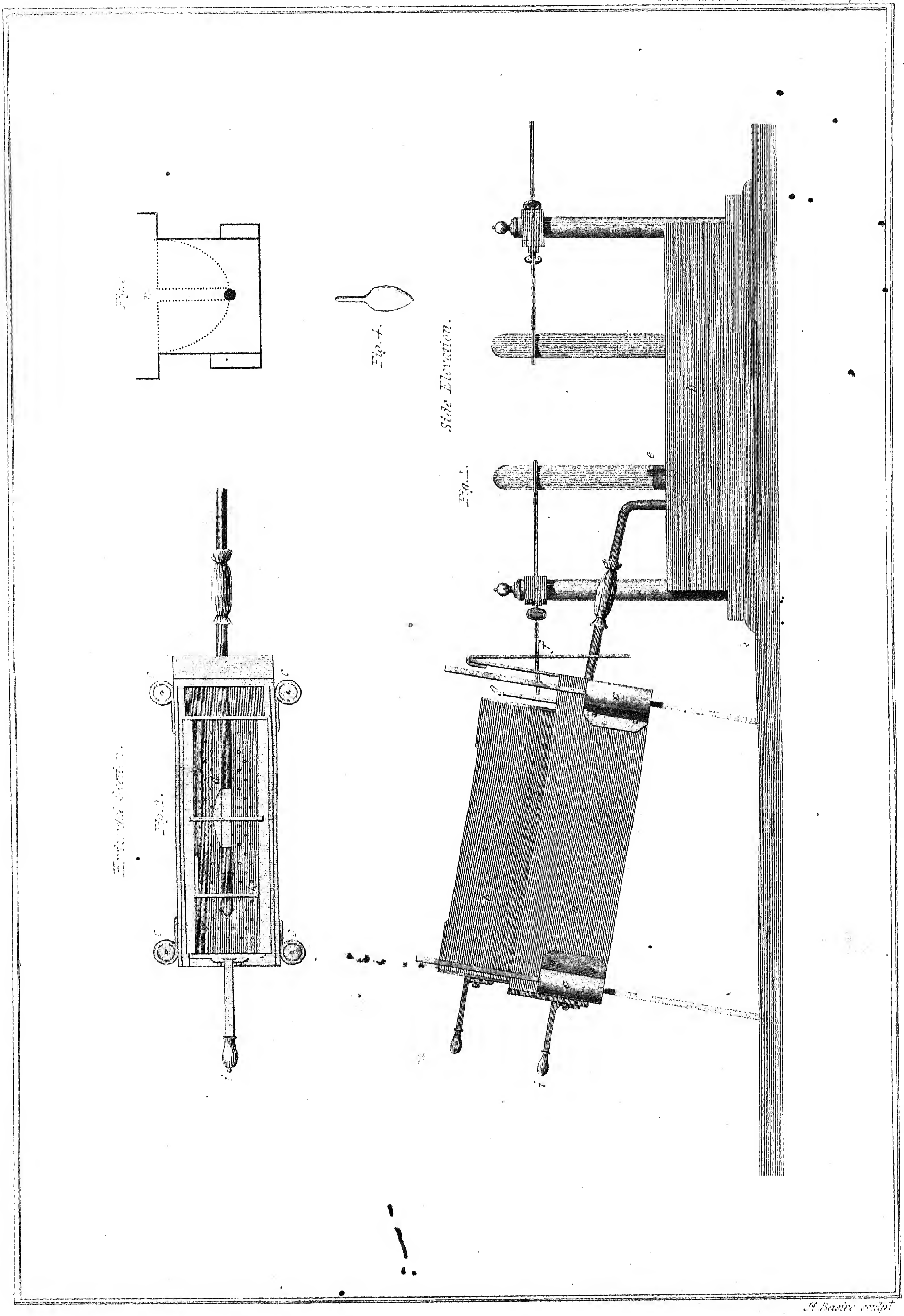

J. Basire sculpt.